CHANGING SCENE

BY F. C. BARTLETT

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The late Prof. Beatrice Edgell (1947) told how the British Psychological Society began its existence at an informal small meeting in London on 24 October 1901. This, therefore, is our Jubilee Year, and it seemed to me that perhaps the most fitting thing that I could try to do this afternoon would be to go back over some of the story of these fifty years of growth and development and to tell the tale of a changing scene. There are indeed, as I am well aware, others who have a greater right to do this than I can have, because their relations with our Society began earlier and have been closer than mine. Sir Cyril Burt, Prof. Pear, Prof. Flugel, Prof. Valentine, Dr E. O. Lewis, I believe the Society's next President, Dr William Brown, and no doubt others as well, were all active members before me, and I owe them and tender them all some apology for my choice of a subject which they could all deal with more fully and more intimately. But it seemed that it would be wrong to ignore a great occasion, and I will attempt to talk in the main only of things that I remember, and people whom I have myself known.

On that day in October 1901 I was just beyond my fifteenth birthday. I cannot recall whether I had ever as yet heard the word 'psychology', but if I had, certainly it had never entered into my wildest dreams that this subject would play any large part in my own life. I was in fact at that time very slowly struggling back to health again after an exceedingly serious illness, and the doctors had prescribed for me, as, later, I heard that they had under not very dissimilar circumstances prescribed for Prof. James Ward, a life in the open air. There I was, far away in the depths of the country, at the north end of the Cotswolds, learning something about the habits of birds and beasts, exploring ancient and overgrown roadways, walking far in all weathers, and, as strength returned, playing every game I could get. Nothing seemed less likely than that I should have a life of study and experiment. And indeed, looking back now, it seems to have been the merest chance remark that sent me to a bookshop a few years later to order a book about logic, and then not much after that, my first two books of psychology: Stout's *Manual*, and the old red-covered first edition of C. S. Myers's *Text-Book of Experimental Psychology*. The die was cast, but if I am to be quite truthful, I must confess that I did not then recognize the significance of the pattern. Of course, I thought that psychology, as it was presented to me in these two famous volumes, was full of interest, and I did a lot of amateur experiments, cutting strips of coloured paper out of pattern books, building up some bits of home-made apparatus, and no doubt bothering and confounding my relatives and friends with a lot of odd questions. But I thought also that there were plenty of other things that called to a young man's fancy.

All this time the British Psychological Society was continuing its small and disputatious meetings that I was to read and hear a lot about later on. How many a time I have regretted that I did not hear W. H. R. Rivers backing Hering against Helmholtz, Myers weightily holding the balance, Ward getting very worked up about the nature of 'black', and McDougall expounding what I still think to have been the best experiments he ever

did, his earlier Cambridge work on colour vision. It was not in fact until about nine, or perhaps ten, years after the beginning that I went up to Cambridge, met Dr E. O. Lewis and Dr Robert Rusk, both of whom treated me with infinite kindness, heard from them stories of the great men among our founders and builders, and then on one, to me, never to be forgotten day, encountered G. F. Stout at a tea party.

It must, I think, have been towards the end of a May Term in Cambridge, for I remember how the sunshine was streaming in through french windows leading to a somewhat enclosed garden, and it was exceedingly hot. There was an unfortunate lady who was mostly silent and obviously nervous. At length she ventured to remark: 'I do like my cup of tea. First it makes you hot and then it makes you cool.' At this Stout exploded with terrific violence. 'I never heard such rubbish', he said. 'First hot and then cold! Nonsense! Absurd! Ridiculous!'

The poor well-meaning lady subsided into complete silence. I was sorry for her. After all, she only said the right thing in a wrong way.

Stout asked me what I was going to do, and then, probably for the first time, I said suddenly that I thought I should try to teach psychology. 'It can't be done,' he said. 'Nobody can teach psychology. It can be learned. It can't be taught.'

Then I thought he was wrong. Now I know what he meant, and I think he was not far off the mark.

The reason I met Stout first, before any of the others, known by name at least to us all, who had more to do with our Society—though he was, of course, an original member and an Honorary Member for many years—was that I lived a little more than a year in Cambridge before I joined St John's College. I went to St John's principally because W. H. R. Rivers was there, and I was being torn between psychology and anthropology, with a bit of a lurking desire for medicine in the background.

This was already after that fateful division of pathways which took place during or following the Torres Straits Expedition, and probably shaped the early history of the Society more decisively than any other single event. This was the division which deflected Myers finally into psychology and Rivers, for many years, away from psychology and into anthropology.

I can bring back to mind very clearly the first time I met Rivers. Already he had reduced the large, somewhat military looking moustache which adorns his earlier photographs. He seemed to me tall, spare, sharp-shouldered, full of nervous though quiet energy. He sat on a sofa in a large room untidy with books and papers. A College 'gyp' brought up tea, two or three bits of bread and butter, and some very sawdusty Madeira cake. He ate nothing and drank only milk and water. He told me then, and many times later, that if I wanted to study anthropology, or, I gathered, do anything else that was any good, I must first spend many hours in the psychological laboratory learning the psycho-physical methods. I did.

Let me now again pay my tribute to C. S. Myers, and to Sir Cyril Burt. Myers was the Director of Cambridge experimental psychology, and Burt was his assistant, who took charge of the practical classes, and ran them with a most delightful mixture of humanity and scientific precision. I doubt if we can have been a very rewarding class. Privately we grumbled and groused like anything. We vowed we would lift no more weights, learn no more nonsense syllables, strike no more tuning forks, cross out no more e's. Colour wheels were more fun. We threw ourselves at the dynamometer and the ergograph in the

hope of beating one another or smashing the apparatus. Optical illusions were temporarily attractive, but we got a bit tired of the Müller-Lyer. Everything that could be was in a strict pattern of psychological method, and now I know it was greatly to our good. But how sweetly we escaped to tachistoscopic experiments on perception and to Burt's own early intelligence tests!

Sometimes now I take down my first experimental note-books and then I realize most clearly of all how changed is the scene since that time forty and more years ago. We worked in the main with the stock-in-trade of Helmholtz and Hering, of Wundt, of Blix, Goldscheider and Von Frey, of G. E. Müller, of Kraepelin. There were a few refreshing excursions into the fields of the Würzburg school, a little of Wilhelm Stern, and a little, a very little, of the new psychology of mental tests, and of Jung's form of word associations. It was Germans, Germans all the way, and if we were going to stick to psychology then to Germany sooner or later we must all surely go.

There were several difficulties that I am afraid are still apt to beset the student of experimental psychology, though I hope less acutely than they did us. When we left the laboratory we promptly dropped it all. What had it to do with our daily life? It was indeed a laboratory game, boring in parts, engrossing in others, but just a laboratory game. And it was not very easy to link up the experiments with the general teaching. When Myers lectured or Burt talked to us it was less difficult. They gave us the classical experimental backgrounds and found time for reflexions upon real life. But when we went to lectures by James Ward and Prof. Dawes Hicks, it was like journeying to a far country.

Ward's first class of the year was a bit of an ordeal. He would come in, suitably late, struggling into an academic gown as he came. He would sit down and take us all in, and then he would go round the class one by one. 'You there. What have you come for?' It did not matter much what we said. He would make it very clear that we were probably wasting our time and certainly wasting his. 'I don't suppose you'll understand anything', he would say. But when, after this, he talked, with great informality and charm, he held us all entranced, whether we understood it (and speaking for myself I frequently did not) or not. Sometimes he asked us for questions, and if we put any he would say, with intense patience, that we had missed all the points which he had most clearly made, and if we did not he would say that anybody who had understood anything would have masses of questions to ask. James Ward became, and I am very happy to be able to say still remains, one of my tremendous heroes. He had done most of his revolutionary work for psychology before this Society was started. Later he spoke to me often about the Society and the people who were shaping its future. Still more he spoke of the *Journal*, which I gathered had been not too bad in its first two years but afterwards had fallen off sadly. He had been one of the original guarantors, and he was mightily afraid that he would lose his money, but I am glad to say that he did not. He was sorely troubled about contemporary developments. Outside of the small range which he would never admit to be more than 'psycho-physics', and a few experiments on memory, he thought most experimental work was trivial, and in a laboratory he was an unhappy man. He knew more than most people realized about the tremendous things that were happening to clinical psychology in Vienna, but he called them 'mad'.

It is, of course, not psychology alone that has changed, but the whole pace and character of social life. The two things have run together as it is inevitable that they should. In the University everybody was more at leisure. There was far less teaching. The only sense

of urgency was internal to whatever subject was being studied. All our professors and many of our lecturers entertained us to lunch at least once a term and kept open house at Sunday tea-time. I suppose the food was nothing out of the way then, but now it makes one's mouth water only to remember it.

And the meetings of this Society: they were mostly on Saturday afternoons and in London, as they are now. They were small and the company rather highly selected in an academic sense. The discussion was generally lively and there was Shand's dinner to follow, a delight to the flesh and the mind. I suppose there are few people to remember Alexander Shand now; tall, perhaps somewhat gaunt, white haired when I knew him and almost always in a spick and span navy blue suit. His interests ranged widely wherever a magnificent literary imagination took him. He seemed to have read everything that was worth while and a lot besides. To listen to his psychological discussions was a great treat, but a greater still to sit by him at dinner.

Into this world, cultivated, rather satisfied, well fed and sociable, and preoccupied with its learned problems, came the threat and then the reality of war. The Society still continued to meet, though less frequently, and generally without the dinner. Soon all of us were absorbed, in one way and another, into the great struggle. I cannot attempt to tell the story in detail but only to pick out those things that were happening which were destined to change the British Psychological Society and the whole teaching and practice of psychology in fundamental ways. I think there were three of these. Two may be called new, but the third was already well under way in this country under the inspiration of Cyril Burt and Prof. Spearman and others.

First, whether it was by accident or by foresight, experimental psychology got tied up with technical physical advances. This was due in the main to the influence of Myers and of Prof. Pear. Submarines were already being attacked by underwater methods of sound transmission, and in this a body known as the Lancashire Anti-Submarine Committee, led by the late Lord Rutherford, took a somewhat emphatic lead. But it soon became clear that not everybody could operate the instruments which were invented and constructed, and Myers and Pear together were able to see that a group of us had the chance to develop what was probably the first really organized effort of team personnel selection, in a technical sense.

Secondly, there was the vast increase of neurotic disorders among the fighting men of the First World War. Myers, Rivers, McDougall, Dr William Brown, Elliot Smith and again Prof. Pear began to show what psychology could do to meet this menace to health and efficiency.

Thirdly,—but perhaps the best way to tell of this will be by a brief story. I remember well how, towards the end of the War, Dr W. V. Bingham came over from America. He brought with him copies of the American Army general intelligence test, and I met him on Saturday afternoon at University College. I am not quite sure who else was there, but certainly there were Myers, and Spearman, and Prof. Edgell, and perhaps Burt. Anyway, Bingham gave us all the tests and as far as I can remember not many of us would have made the grade. We felt a bit doubtful about them, but it was clear enough that they had come in some form or other to stay, and we must be prepared to do something about it. After all Spearman and Burt particularly had little enough to learn in this field.

It seemed then and for many years following that these three developments—the technological link with the physical sciences, the strong urge to base everything in psychology

on clinical analysis, and the intelligence test movement with its active association with statistics—could not well be brought together. Each represented an approach which appeared somehow a bit distasteful, or at least foreign to anybody who preferred to adopt one of the others. But during that same period another development took place in this country, the fundamental implications of which have not yet, I think, been fully realized. It was due to Sir Henry Head and Dr Gordon Holmes.

Head is not easy to put into a word portrait. He was a man of most brilliant parts, with a glancing intelligence that shot off in all directions, with remarkable histrionic powers, equally at home in literary, artistic and scientific circles. More than most people he combined a terrific capacity for rather impatient experiment with an amazingly fruitful power of guessing. He was a large man, florid to look at in those days, flamboyant, a bit of a buccaneer. He could easily have been mistaken for an artist or a poet, and, indeed, he had published a book of poetry which many thought very nearly first rate. Once when he was having dessert after a feast at St John's College in the Combination Room, where the long table was lit by tall candles in their gleaming silver stands, completely carried away and waving his arms in magnificent gesture he declaimed, 'The table lit by a thousand candles'. Rivers's old 'gyp' was passing by at the time. He stopped, bent over: 'Two hundred and fifty seven, sir', he said, 'to be exact.'

It was during the First World War that Head began effectively his work on aphasia, famous at the time, though now, I am sorry to say, apparently less regarded. Most of his patients were soldiers with gun-shot wounds in the head but otherwise healthy. His method was a highly original combination of experiment, test and clinical analysis and assessment. From time to time Head used to invite me to go up to London for discussion, the discussion being in fact almost all on one side. He put me through all his tests and experiments, and I must say that he made it hard to get any results different from the ones he knew to be right. But never mind, they were genuine experiments, and in his work the three main developments of these war years came together.

Prof. Edgell recorded the factual history of the great changes that came to this Society in the years between the two Great Wars. What I will try to do is to tell the story more from the inside and in terms of some of the principal personalities concerned.

W. H. R. Rivers was back with a bang right in the middle of the psychological scene. All those reclusive habits which, when I first knew him, he seemed to be hanging on to with grim determination, he gaily flung to the winds. He knew everybody and went everywhere. He produced paper after paper, all of them theoretical, all sweeping and dogmatic, and all of them written out in handwriting that was almost completely unintelligible to anybody except himself, but in language and with ideas that everybody thought he could understand. By bent of mind and character he was really no organizer of people—though in his own way he tried his hand at this quite a lot in these days—but an untiring organizer of ideas. He had a vast and humanizing influence on the intellectual outlook and preoccupations of the Society, but none at all, save only indirectly, upon its shape and outward constitution.

Yet the indirect influence was great enough. It came through C. S. Myers, who was a little younger and then in the habit of discussing all his schemes with Rivers. Both of them in those days saw psychology finding its place in every form of human activity—medical, industrial, technical, political, literary and artistic, and educational—and gathering into its fold all manner of people of intelligence and good will. They shared the same

vision, but whereas Rivers passed it on to others in a personal way, Myers had the patience, the skill and the genius to give it an institutional form.

In 1922 there was an International Conference of Psychology at Oxford. Anybody who cares to look up the published record of the Proceedings will see how much it forecasted the impending expansion both of this Society and of psychological interests generally in this country. Myers was its President and was in fact behind the scenes in every part of its organization. Everything that became a live sectional interest of the Society had its place there: the general and experimental, the industrial, the medical, the social and the aesthetic. The manner of it was also significant. The appeal was wide and unlimited, but the leading performers were in fact selected unobtrusively but with great tact and skill. The German dominance had gone. It is true that a small band of Germans were there and made a profound impression. But they were themselves in revolt against the classic German tradition; they were Wertheimer and Kohler and Koffka. If one looks at this Conference as a whole it is easy to see that its main trend was either wholly away from the academic and intellectualized psychology of the past, or at least towards an effort to humanize all the rather arid traditions.

In these days Myers was restless, unhappy about the state and status of psychology but unswervingly loyal to what he regarded as its true claims, and full of plans. The British Psychological Society must grow. It had to grow in order to maintain its *Journal*, but apart from that it must grow to match the rapidly expanding interests in animal and human behaviour, which, as he saw before most people, must profoundly modify psychological study and research, if not within our control, then outside of it and probably to its own cost. Of course, he was not alone. Spearman, Burt, Pear, Drever, Prof. Edgell, Flugel and a good many others played their part as well, but I think we would all acknowledge that he was the chief architect.

This was the period during which the three principal lines of psychological interest which grew up during the First World War proceeded in extremely rapid, but in the main, separate, growth. Above all, maybe, they were the years during which almost every psychologist proclaimed himself an anti-intellectualist. Feeling, emotion and instinct were the master keys, and in many quarters they were melted down into a single master key in the pattern provided by the genius of Sigmund Freud. Almost equally they were the years of tests, and selection by test for all manner of practical activities, and of the statistical analysis of test results which, oddly enough in spite of all clinical bias, seemed in many ways to be holding fast to an intellectualist basis. Both of these movements naturally took many psychologists, especially in America, into a world of society, and methods of social investigation were rapidly shaped which borrowed both from the clinical and from the test pattern of approach. All this time I think experiment proper in psychology was a little unhappy, alternatively becoming immersed in more and more detail of the classical type, and trying to break out of the laboratory but uncertain how it could do so without doing violence to the established principals of scientific method.

All this was bound to be reflected in the proceedings and organization of the Society. It grew rapidly in size, as everybody knows, but I fancy that its sectional form tended rather to divide than to unite its interests, for each section shared in the movement common now to all science and most knowledge and became more and more specialized. Professional problems and questions of status and reward, unknown to the Society in the earlier days, became inevitably more insistent, and however much those of us born in

easier times were tempted to disregard them, we could not deny that they are important and must somehow be solved.

Then came the Second World War. We are too near to this as yet to assess fully its impact upon psychology and the Society. But one thing seems to me to be already clear enough. It is that the technical achievements upon which the conduct of almost all military operations now depend helped both to re-establish experiment again as the method of scientific advance without which no sure progress can be made, and also to bring the laboratory into closer touch than ever before with the world outside. It now seems that the emotionalism of the twenties and thirties was as one-sided and insufficient as the intellectualism that preceded it. It even seems that social problems cannot be finally or satisfactorily solved by just thinking about them and adumbrating general ideas and schemes. We are perhaps learning how to combine the clinical with the experimental; we are perhaps coming to see that no matter how sectional and specialized our particular interests may be they must come together in a basic methodology which can unite explorers in all the fields of human knowledge.

What all this may mean for the British Psychological Society nobody can yet tell. But when I try to compare especially the psychological laboratory of to-day with the environment in which I began to work, I see a tremendous change of scene. The sense of leisure has gone but with it also, to a large degree, though not yet entirely, a sense of artificiality. Psychology, especially on the experimental side, is no longer just hoping to be a science based upon a little anatomy and physiology and a lot of speculation. It has its widely acknowledged link with all the other sciences and with all the practical activities of man, individual and social. It demands an equipment far greater, more specialized, and at the same time more all round, than we ever had, and it offers chances of which we never dreamed. But above everything else, as I see it, the old puppet subjects of the early German psychologists have had their time. They have played many a good play. Their strings are broken. Their commendatory epitaphs can be written and they can be put away in their boxes. In their places we will have real people.

Reference

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THE BOUNDARIES BETWEEN THE PERSON AND THE WORLD*

By GARDNER MURPHY

The Menninger Foundation, Topeka, Kansas

In contrast to the widespread opinion that organism and environment (or person and world) can be sharply distinguished, the thesis is propounded that the one often melts into the other. Data from physiology, genetics, behaviour psychology, the psychology of immediate experience, social psychology and parapsychology all suggest that the 'gradient' from person to world may be either 'steep' or very gradual.

I. Introduction

In ancient Greece and in ancient India, and throughout the history of the two civilizations which have grown from them, there have been several perennial answers to the questions of the relation of the person to his world. While psychology gathers new data and develops new viewpoints, it still seems to revolve mainly within the classical orbit set by these ancient philosophies, rather than attempting in the name of science basic and fresh formulations in relation to this problem. A central aspect to be treated here is the question of boundaries—spatial, temporal, functional and conceptual—which mark off the realities and events called personal from those called extra-personal. To suggest the range of possible ways in which we may conceptualize the relation of the person to the world may perhaps help to make clear what we do not at present understand about the nature of persons, and may help to focus experimental, clinical, comparative and other research procedures toward the attainment of a clearer and more workable formulation of the possibilities.

The boundaries between person and world may be viewed in terms of physical, biological, psychological and social science.

II. Physical and biological boundaries

From Democritus to Schrödinger (1945) the physical view suggests that what is inside man is essentially like what is outside; he is a bit of the cosmos, duplicating it in substance, frequently also in structure.

The world as envisaged, for example, by Democritus, consists of particles, some of which are bunched in the form of organic beings, and some of the particles within such sentient beings make up the 'mind' of the person. From this conception, through Lucretius, Hobbes, La Mettrie, and the monistic systems of to-day, an essential identity of thought can be observed. From this point of view, there is a clear and obvious boundary between person and non-person, namely, the capsule, the envelope, the spatial boundary, essentially the skin of the living individual. At times, the monist espousing such a view glimpses a larger and more complex relationship, as when La Mettrie, for example, compares man to fire, the process of respiration involving a subtle and fluid interaction between person and world. Though the cosmos permits momentary isolation of small portions of itself, biochemistry has made clear, for example, that in vital processes

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like respiration it is meaningless to define a sharp boundary. Thus, in Henderson's 'nomogram' of the blood (1917), the living system within the skin is not sharply separable from the non-living system in which it is bathed; the air in the red-blood cells can be regarded either as part of us or as part of our environment, indifferently (Northrup, 1931). The gradient between man and world is a gentle one indeed.

Biological research nevertheless forces our attention to an apparent paradox: the fact that the organism is ultimately inseparable from its environment means that its very existence depends upon its obedience to principles quite different from those which generally obtain outside it. In an 'open system', for example, specific processes, such as the maintenance of homeostatic balance, prevent the irruption of the outer form of order into the protected inner realm within which a fundamentally different kind of order prevails. As C. D. Broad (1925) phrases the matter, we find, in contrast to the principle of substantive vitalism, the operation of 'emergent vitalism', a conception according to which the same material particles which are found in the universe at large are found under conditions of such organization and at a level of such emergence as makes vital phenomena essentially different from those occurring in the outer world. We may thus, if we choose, avoid dualism of substance, yet provide dualism of functions.

Thus, despite this conception of homogeneity there are constant attempts to develop some plausible basis for the fundamental struggle of the individual against the world. The conception of homeostatic balance enables us to envisage ways in which particles essentially like those of the cosmos at large may, nevertheless, as it were, fight against the disintegrating forces of the cosmos, maintaining for a while their own tightly structured internal coherence. Lifting this conception to the cultural and ideal sphere, we have the view propounded by Thomas Huxley's *Evolution and Ethics* (1894), and Bertrand Russell's *The Free Man's Worship* (1923), in which the value system of human beings negates, opposes and stands in stark opposition to the valuelessness and impersonality of the world.

But it will surely not suffice for science to declare only that we are both one with the environment and also embattled against it. The empirical issue is to ascertain just when and how far Henderson's conception of the *gentle gradient* holds good, when and how far the *sharp gradient* of homeostatic isolation prevails, and just when and how our relative independence of the environment is to be defined. Leeper (1954) points out that the term gradient is used here for no single physical continuum. Rather, I refer here to the continuum of *degrees of interdependence*. Ultimately, I suppose, our thinking about degrees of interdependence may become strictly quantitative, in which case we could refer to covariation.

III. PSYCHOLOGICAL BOUNDARIES

Turning now to the viewpoint of psychology, we find the gradient between person and world ordinarily treated as very steep. We speak of individual and environment as two utterly distinct things, with a sharply defined boundary. Our diagrams and our conceptual schemata show no region of transition; everything is either inside or outside the organism. Yet despite all this, contemporary psychology offers at least three other possible approaches.

First, the Gestalt conception of inner structure, such as the structure of thought or the structure of personality, in contrast with a lower degree of order in the environment, reveals a parallel with the sharp gradient of homeostatic balance.

The concept of isomorphism is an effort to give the individual a structure and an autonomy, while providing resonance of inner to outer reality. The term *isomorphism* has been used in two senses: first, to denote the correspondence of mental structure with the structure of neural response; and secondly, to denote the correspondence of inner structure with environmental structure. It is to the latter that I refer. Isomorphism, indeed, is a special case in which inner structure mirrors outer structure, not through capitulation of the inner to the outer, but through their capacity for attunement. Compare here the the biophysicist, Selig Hecht, with the Gestaltist, Arnheim (1951). Hecht (date unknown) is quoted as saying: 'The organism does not adapt itself to the environment; the environment adapts the organism to itself.' For Arnheim, on the contrary, isomorphism is that structural correspondence between the physical environment and the psycho-physical processes in the person which makes adequate reaction to the world possible. In relation to music he writes of 'the structural kinship between expressive effects and specific patterns of rhythm, pitch, harmony, timbre, or volume'.

A second concept characteristic of modern psychology presupposes formal independence of person from world, combined with absolute dependence through the ceaseless give-and-take tennis game of acts and signals, the world of cybernetics and of information theory, of Brunswik's navigation through a sea of uncertainties (1943), Leeper's complex contacts with heterogeneous reality (1954), Sears' dyadic functions (1951)—a world where reality is reciprocity, not the world of events-within-the-individual-person which our self-reliant grandparents knew.

It is not ordinarily noted here that in many such cases the interdependence of part of organism no. 1, say its vocal cords, and a part of organism no. 2, say its ears, is very much closer than the interdependence of most parts of organism no. 1. If this be mystical, make the most of it. Functional interdependence is what we are now studying, and the lines of functional interdependence do not neatly run in closed circuits inside one individual.

The third psychological conception goes even further. By all means the most original and radical of the modern efforts to transcend these traditional dichotomies, is the field theory of Kurt Lewin (1935). Incomplete and in some ways self-contradictory at the time of his death, this was, nevertheless, a brilliant adaptation of the general scientific conception of the field as developed first in physics and then in biology, transported into pyschology to indicate the relative inseparability of person and world. As a matter of fact, it is only in Lewin that one refuses to accept the ancient question: Are we one with the world or isolated from it? Neither, says Lewin, and proceeds to show how those aspects of a response which are properly called personal and those aspects which are properly anchored in the environment come to meet one another in one organized field dynamic. In the 'life-space' of Lewin there need be no fixed division into outer and inner.

In some types of field theory, sketched, for example, by Angyal (1941) and by Sullivan (1953), a unified world of person and environment yields at some points a gradient as gentle as that of Henderson. I have elsewhere tried to show that in many empirical problems like the preparation of a life history, the field conception, with its fusion of outer and inner, notably improves our powers of explanation and prediction. While all concepts of the nature of the individual encounter deep-seated resistance, the notion that we tend to melt into our environment encounters a resistance which is perhaps proportional to our culturally engrained conviction that there *must* be something which is utterly our own.

IV. SELFHOOD

Up to this point we have been able to use the terms *person*, *organism* and *individual* more or less interchangeably. This is because we have been considering ways of attempting to view objectively the relations of organism and environment. There is, however, another issue which may perhaps be of interest in connexion with the problem of objective boundaries, namely, the problem of the way in which the barrier is viewed by the experiencing individual. This is the problem of our awareness of our own individuality, commonly called the consciousness of self. Classically, throughout both Indian and Western traditions, the thing known as the self, the observed me, develops like an object of experience, comparable in all fundamental respects to the development of other percepts. The newborn have no clear awareness of self; but gradually through the observation of body surfaces, muscular contractions, the sound of the voice, cathexis upon these physical entities and constant comparison of them with other entities observed in other persons, an empirical self is woven and given abiding structure. This self may undergo transformations and elaborations, but it represents a relative continuity from the cradle to the grave, is given a name, is given a social position and sense of responsibility, and around it in time cluster the goals, values, norms and ideals which give meaningful purposive continuity to life. It is fundamental to most such theories that the self becomes a loved object, Freud's narcissism being the clearest of the Western formulations of such self-love.

There is, however, constant reciprocity between the image of the self and the image of others; and this means that fundamentally, from the point of view of an empirical psychology, the contours of the self are often blurred, and the distinction between the self and non-self indistinct. This is fully recognized, both in associationist doctrine, in the empirical studies of the school of Piaget, and in the psychoanalytic system. As a matter of fact, the question arises, as one studies both Piaget and Freud, whether the self ever does become sharply separated from the perceived selves of other individuals and consequently whether a sharp delineation of the self is ever achieved. One might set up a hypothetical dissociation so profound that selfhood is separated by a deep gulf from all else in the world. This is hard to imagine in view of the endless social commerce of selves which have to be perceived as phases of a process of social interaction in a context of wholeness. Yet in certain types of mental illness, this extreme separation can be observed or hypothecated, while at the other extreme other types of illness involve an incapacity to separate, even relatively, the regions definable as self. Empirically, then, there are *degrees* of independence of self from non-self.

There exist, however, in all known societies a great many phenomena of awareness in which self is not distinctly marked off or in which it has properties quite different from the self as we know it. There even appear to be some completely selfless experiences existing momentarily, and experiences of complete immolation or identification in group or cosmic processes. Some of these experiences have been studied in early childhood, through the clinical methods of modern psychiatry, through the methods of ethnology, and occasionally through the methods of experimental psychology, especially in the case of depersonalized hypnotic states. Such investigation shows that the sharp isolation of the individual is merely one special case of consciousness, like Euclidian geometry among non-Euclidian systems. There is, moreover, some experimental material on the conditions

which lead to such impersonal states and which throw light on their broader significance. There is, of course, an abundant opportunity here for self-deception, and it would be ridiculous to maintain that the person's experience of boundaries between self and non-self is a safe guide to the real situation. He may be a psychotic whose illness consists in large part of his inability to accept the real boundaries. But if the fusion of selves is experienced by some persons as a reality, this constitutes a psychological problem which may merit study, along with the problem of the nature of the objective boundaries.

This soon leads over a bridge which to many will appear to be perilous indeed. For selfhood is seen from all these vantage points to be capable of only a relative definition. It represents a reality which is only partially detachable from a flux of experience not sharply organized around the self. This re-opens some issues raised in ancient Greece and in ancient India regarding the real separation of one person from another. This question remainds one of John Donne's words: 'No man is an island.' Such a conception, developed by mystics in both the Eastern and Western traditions, has pointed to a type of psychological reality which is not anchored in any single self. It has insisted that there is not only the reciprocity of self and social environment, but at times a true and intimate fusion of selves, of personal identities. This view has usually been conceived in religious terms through identification with a Deity, or as in the Greek mystery religions, as identity of the fellow worshippers in their identification with the Deity. There is, however, much more here than a system of religious and philosophical possibilities. There are challenging psychological problems which have been posed by studies of sleep, trance, hypnosis, ecstasy, the phenomena of 'possession' described in the ethnologists' and historians' records. The empirical problem of the degree to which selfhood can disappear leads on into the problem of the interaction between two or more selves in which the isolating sense of individuality tends to be lost in a sense of communion.

Please let me repeat for emphasis: I do not mean to suggest that the individual's sheer belief that he is dependent on others, or independent of others, should be taken as anything more than just a belief. He may, of course, actually be more dependent on others and on the environment than he thinks he is, or he may be less dependent. My point, rather, is that the problem of dividing the world into experienced self and non-self is a problem that needs to be pursued conjointly with the problem of the nature of the objective biological gradient.

It might be thought that an appeal for a study of conditions of selfhood with very fluid and indistinct boundaries is essentially an appeal for a study of queer, bizarre or abnormal states. On the contrary, it seems very inappropriate to use normative concepts, terms of evaluation and derogation, when dealing with states of consciousness about which we know so little, and which in the nature of the case appear alien to our more customary ways of thinking. William James remarked: 'The world of our present consciousness is only one out of many worlds of consciousness that exist.' I put to you, therefore, the question: Is it better for psychology to finish in all completeness the study of one kind of awareness, or to strive for an openness towards the possibility of areas of experience as yet unexplored?

I believe that our reluctance to investigate these phenomena is connected prominently with the vigorous individualism of Western history, and connected likewise with the feeling that our long cherished emancipation from philosophy and religion will somehow exempt us from coping with problems of this order of complexity. As a matter of fact, the

same motives and the fear of losing caste among positivist scientists has interfered on a huge scale, even with the investigation of relatively simple and objective laboratory phenomena. Such laboratory phenomena, for example, are now available in the field of parapsychology, meeting the challenge for strict experimental control. A huge range of empirical materials is now at hand, interconnected by a very considerable body of coherent systematic conceptualization. Most of this conceptualization is unknown to psychologists, indeed even the empirical material is largely unknown One often hears the remark that one might begin to believe in the authenticity of the many experiments in telepathy to which no objection has been pointed out, if there were some sort of coherent theory regarding it. Yet several coherent theories have already been developed in intimate relation with the experimental facts, and progress in development of repeatability of experiments goes hand and hand with clarification of the conceptual systems. Psychologists are, for the most part, unfamiliar with this material, and, as scientists, I think one might say quite literally, afraid that there might be some truth or some value in these lines of inquiry. Science can seldom afford to be afraid of investigation, and in this instance, as in so many others in the history of science, fear can actually cripple the types of investigation which would give true unity and resolution to what have proved otherwise to be insoluble problems.

In the experiments of Soal and his collaborators (Soal & Goldney, 1943; Soal & Bateman, 1954), it has been possible over a span of several years to observe meaningful variation in telepathic scoring level between subjects working in different rooms, with independent observers in both rooms, with randomized stimulus materials and elaborately careful and cautious statistical treatment. The phenomena were not only in conformity with reasonable psychological hypotheses regarding the dynamics of the process, but exemplified the specific point here suggested, namely, the interpersonal nature of the telepathic phenomena. In some experimental series, two separate individuals working independently each possessed some information about the stimulus material, but neither one knew its whole character. A third person, *C*, the sensitive subject, was able to piece together the material, so that this mind *C* actually represented, at the time, an integration of the contents of three minds—*A*'s, *B*'s, and his own. I ask only that you take this as a challenge to read or to think. It is perhaps of interest that many of the phenomena suggested by unconventional ways of looking at the person raise essentially the same kind of question, namely, the question where one person begins and another ends.

V. Social boundaries

Finally, in the social science approach we have been pushed by more and more evidence in recent years to admit the profound interdependence of members of society, the full membership character of individuals in their group, and the reality of processes which are fully interpersonal, not simply the accretion of the behaviour of distinct individuals. Again societies differ in the degree of autonomy allowed to the individual, with autonomy varying from great to little, but never suggesting the reality of any wholly self-sufficient person, any more than psychology, biology or physics could allow such absolute self-sufficiency.

How did we manage to forget so much of this in erecting a pedestal upon which the individual was to stand—poor Simeon Stylites, alone in his elevation above the group?

We forgot, perhaps, because we are parts of the Western conception of individuality, made razor-sharp by the Commercial and Industrial Revolutions. We grow to-day in actuality more and more dependent upon one another, but the rugged individualism of a tradition is still spread all over our psychology. Is it really necessary to give the Western conception of individuality not only the right of way, but actually the sole berth in the establishment of psychological science? Is there a possibility that by greater openness, we could recognize that in Western psychology we have only one of an infinite variety of possible relations of self to world?

VI. The need for research

Not, of course, that we are fully consistent. Perhaps we might conclude that some aspects of persons are relatively sharply bounded, others the reverse. Paradoxically, we give heroic stature both to the supreme Whitman-like 'individualist' and to him, like Gandhi, who is 'selfless' in devotion to a social goal. In an era in which humanity can hardly expect to survive without fuller knowledge of the biosocial value of individualistic and social impulses, research is needed into the sources both of man's need to isolate himself and of his need to soften his individuality in the presences of his fellow men and of the sea and the stars; we need to understand how his culturally moulded selfhood sometimes leads him to make the gradient seem much *less steep*, sometimes much *steeper*, than it is.

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THE PRINCIPLE OF INDIVIDUATION AND THE CO-ORDINATES OF CONDUCT*

By J. C. RAVEN

From the Department of Psychological Research, The Crichton Royal, Dumfries

Theories can be of two kinds, 'disciplines of thought' or 'speculative hypotheses'. The 'principle of individuation' provides a discipline of thought capable of embracing the mathematical equation, the dimensions of physics, biological interaction between individual and environment, and the psychosomatic problems encountered in medicine and in so-called para-psychology. It indicates not only the lines along which divergent schools of psychological thought and inquiry have developed; it gives order and clarity to the various interpretations of conduct suggested, and the hypotheses put forward for experimental investigation without which the collection of data becomes confused and unproductive. It also gives the statistical treatment of quantitative studies their appropriate place and function as a means of determining the degree to which any qualities assessed are in fact genuine co-ordinates of conduct and dimensions of personality.

I. Introduction

My paper is concerned with the problem of formulating the appropriate questions to ask in order to provide an adequate psychological account of human conduct. In a former paper (Raven, 1951) I have tried to show that if applied psychology is to advance, there must be a corresponding advance in psychological theory. Nowhere is this more urgent than in the fields of clinical and abnormal psychology.

We have, I think, to recognize two distinct kinds of theory. We can call the first kind 'pure theories' or 'disciplines of thought'; we can call the second kind 'speculative theories'—that is, 'interpretations' of experience in the light of past events and 'hypotheses' concerning future events which can be verified by observation.

Whereas 'speculative theories' require verification, 'disciplines of thought' concern the processes of reasoning from accepted premises to rational conclusions, if not regardless of facts, at least regardless of all facts outside the universe of discourse agreed to at the outset. Mathematics, for example, is a discipline of thought. As Guilford has pointed out, mathematics is essentially theory without speculation. It is based on the *a priori* premises that one and one shall equal in all respects two, and that any observations of one and one not making exactly two (as happens, for example, in all vital processes) shall be excluded at the outset from the universe of discourse. For the sake of argument it is also agreed that qualitative differences between individuals shall be systematically ignored. In dealing with the psychological problems, the limitations of mathematics are therefore obvious.†

In order to give a systematic account of living activity, the psychologist needs to begin a stage earlier in his experience than the physical scientist. As Lewin points out, the psychologist must give a systematic account of the processes of individuation themselves, as well as of the individual entities which emerge from their surroundings, and can

* Based on a paper read at the Annual Conference of the British Psychological Society, 1955.

† Because we find $fx \rightleftharpoons Fy$, it does not necessarily follow that f and F can be adequately formulated mathematically, i.e. in purely additive terms. Nor does it follow that there is any function ϕx which equals in *all* respects ΦY. Mathematics therefore provides useful symbols, but suggests a misleading discipline of thought.

subsequently be compared and counted. It follows that for the psychologist to adopt the purely objective discipline of thought appropriate to the physical scientist, is for him a confession of defeat—a confession that he is unable to give a consistent account of what he actually experiences, and that as a result, psychology must remain for him a branch of the physical sciences or of sociology, instead of what it can, and I think must, necessarily be, namely, the science of individuation itself.

II. The principle of individuation

Gestalt psychologists following Bergson have shown that we apprehend experience as a whole before we perceive the parts constituting it, and that we go on from the whole to the parts by a process of pragmatic analysis into figure and ground, individual and environment, self and non-self, as interrelated opposites. Normally also, as awareness increases, an experience tends to differentiate into a knowledge of oneself attending to particular characters of the experience, first as interrelated qualities, and finally as individual entities, which can be thought about separately as if existing apart from their surroundings. 'Individuation' may therefore be defined as *the emergence of individual entities, objects and modes of experience and behaviour as systematically organized parts of larger unitary wholes, from which, by a process of differentiation, they acquire individual characteristics.**

It can be seen that the appropriate psychological discipline of thought consists in asking this question: Of all the qualities by which individuals are distinguished one from another, which of them are genuinely opposite in nature, and what, therefore, is the minimum number of antitheses (or 'co-ordinates') required for a systematic account of human conduct?

III. The co-ordinates of conduct

(A) *Introverted-extraverted awareness*

For the purpose of description and assessment, we must obviously begin with the universal tendency of experience to differentiate into subject and object as opposite poles. Even in the physical sciences the illusion of pure objectivity is rapidly thinning, and the part played by the observer is coming more and more into view. We shall never succeed in giving an adequate account of human personality by neglecting either subject or object, for both play decisive roles in any idea we form of ourselves or of the world we live in, and any attempt to ignore either in our anxiety to emphasize the importance of the other must eventually lead to disaster.

Introversion and extraversion are perhaps the two opposite poles of experience most frequently discussed, and the dimension of personality most often assessed in one way or another, either from what a person says or from other ways in which he behaves. Nevertheless, people differ not only in the degree to which their attention appears to be centred in the subjective world of inner reality, or in the objective world of outer reality; they differ also in the degree to which these two antitheses of observation, within and without, reciprocate each other, so that a person achieves his desires in harmony with his surroundings. We need therefore to think of subject and object as a pair of interrelated opposites, differentiating from each other to form an essential co-ordinate of conduct, while

* The *Oxford Dictionary* defines individuation as 'the processes leading to individual existence'.

from the work of Jung it can be seen that speculative interpretations and testable hypotheses concern the degree to which a person seems aware of his needs and his surroundings, and is able to adapt his behaviour accordingly.

Briefly, we ask: How far is a person subjectively conscious or unconscious of his own needs; to what extent is he objectively aware or oblivious of his surroundings, and, as a result, how far does he seem able to act as he desires in sympathy with his environment? In short, how far is a person able to find what he wants?

(B) *Intellectual organization*

Simultaneously with, but nevertheless independently of, and in this sense 'orthogonal' to, the differentiation of experience into individual and environment, there emerge intellectual judgements concerning the relations between them, so that the degree to which a person satisfies his needs in any given environment depends in turn upon his capacity for rational thought, as a result of which whatever he observes acquires a degree of systematic organization. How far systematic organization is a product of intellectual activity itself, or is an intrinsic characteristic of natural phenomena, is a metaphysical question concerning which we need only point out that it is a universal co-ordinate of knowledge. Whether we look inwards to the realm of thought, or outwards to the world of things, as far as we perceive individual entities and conceive relations between them, all knowledge is systematically organized. Differences concern only the level of the systematic organization observed.

As far as theoretical principles are concerned, we already have the contributions of Spearman and Wertheimer, to mention only two, while speculative interpretations and testable hypotheses concern the degree to which a person's conduct manifests intellectual organization.

The question is this: Compared with others, what is a person's present capacity for rational judgement; what abilities has he at his disposal, and to what extent does he use them discriminately; or if he does not, what difficulties and disabilities does he encounter? In short, how far does he act intelligently?

(C) *Affective appreciation*

'Orthogonal' to both the co-ordinate of introverted-extraverted awareness, and also intellectual organization, we have again the co-ordinate of affective appreciation, in the sense that the subjective world of thought and the objective world of things alike tend always to be apprehended as being to some degree 'pleasant' or 'unpleasant', 'attractive' or 'repulsive', 'good' or 'bad', irrespective of the degree to which either is apprehended as systematically organized entities.

Apart from the work of Shand and Scheler, the role of affective appreciation in the genesis of character has been emphasized chiefly by psycho-analysts. Unfortunately, in contrasting the 'pleasure principle' with the 'reality principle', Freud confused standards of personal judgement with standards of social approval, and telescoped pleasant-unpleasant feelings with subject-object relations. As far as direct experience is concerned, affective appreciation remains the basis of value judgements throughout life. The change which takes place is that as maturity is reached, an increasingly high value is placed on

social approval, until for many people it is adopted as the highest value of all, and the absolute standard of right and wrong, reality and fantasy.* As a co-ordinate of conduct, the psychological study of value judgements is still in its infancy. At present we can say little more than that goodness and badness form an essential co-ordinate of conduct, determining what we attend to as important, ignore as valueless, or reject as evil; while speculation concerns the degree to which a person's conduct reflects his affective appreciation and critical evaluation of events.

Briefly we ask: What does a person tend to seek, enjoy, and appreciate as good; what does he turn away from, depreciate or fear as bad; in making his judgements what occupies his thoughts, what does he ignore, and what are his standards in assessing events and organizing his conduct, or how far does his conduct reflect a lack of affective appreciation and faulty evaluation of events? In short, what interests him?

(D) *Persistent change*

Finally, whether we consider the inner world of thought or the outer world of things, their systematic organization or their relative value for us, continuous change is seen to be an essential co-ordinate of all experience. When we turn our attention inwards, this continuous flow of change tends to be apprehended as conative activity, or persistent striving towards future ends of action in the light of past experience. When we turn our attention outwards, it tends to be apprehended more as some form of physical energy, or organic function. The more awareness becomes extended in time, so that we recall the past and anticipate the future, the more we form definite ideas of ourselves persisting through change, and endeavouring to achieve future ends of action in the light of past events, so that the past is not merely recalled and repeated, it is also reorganized with respect to future events, and what we are attempting to achieve or become.

Here we have the psychological contributions of McDougall, Piaget and Gardner Murphy, although problems of enduring change with time have so far been dealt with chiefly by people like Bergson, Whitehead and Woodger. Speculative interpretation and hypothesis concern the degree to which a person's conduct shows persistence of determined effort in changing circumstances.

The question is this: How far does a person's conduct at any given time manifest well-defined intentions; and in pursuing his objectives, does he adapt his acquired abilities in ways which steadily develop his own individual integrity through time and change; or does his conduct show excessive fixity or mobility of ideas and rigidity or impulsiveness of action, resulting in fragmentation of thought, autonomous behaviour patterns, and instability of character? In short, is his behaviour self-consistent?

IV. The theory of opposites as a psychological discipline of thought

It would seem that the principle of individuation, and the pairs of related opposites which tend as a result to emerge as the co-ordinates of thought and behaviour, provide psychology with a consistent series of questions concerning human conduct capable of linking together the present divergent schools of psychological theory and investigation. In dealing with psychosomatic problems in general—even in dealing with the abnormal extension of awareness in time and between individuals encountered in the field of so-

* Hence the description of 'reality' as 'the fantasies we share with other people'.

called 'para-psychology'—the main value of discipline of thought based on the individuation of experience into related opposites is that it reorientates the questions we ask in a way which makes it possible to answer them more satisfactorily. Instead of *starting* with the assumption that the mind and the body are two distinct entities, and *then* asking how they run parallel, interact, or arise epiphenomenally one out of the other, we begin to ask quite different, and, I suggest, much more profitable questions, both speculatively and also for experimental investigation.

Academically, we begin to ask: If a living individual is an evolving form of systematically organized activity, why do we imagine two separate entities, body and mind, distinct from each other, yet related with each other in ways we find it difficult to explain and impossible to agree about? In other words, how do we distinguish mental activity from physical activity, and what do we assume to be the essential differences between them?

From our various ideas of mind and body, it would seem that the different concepts we form depend upon the different qualities of conduct contrasted as opposite in nature. In general, the more a physical body of any kind behaves in such a way that it maintains its own individual identity by *adapting to* environmental changes, the more we think its behaviour expresses mental activity, and that it has a mind or will of its own. Conversely, the more any form of behaviour is observed to endure as an organized form of activity *regardless of* environmental changes, the more we tend to think of it as an organic structure, showing an inertia characteristic of a physical body. As a result we come to think of a *material body*, and to define it with some accuracy, as *an enduring form of organized activity*, while we come to think of *mental activity*, and ultimately define it with some accuracy, as *those changes by which an individual adapts to present situations in relation to its own past and future.*

In practice, and particularly in clinical work, the question therefore becomes this: How far does a person's thought and behaviour appear to be an original appreciative response harmoniously related to his surroundings; or how far does it appear to be an established organization of activity, enduring regardless of environmental changes and personal desires. In other words, how far does his conduct appear psychogenetically determined, or determined by his organic structure?

It can be seen that we are dealing not so much with two distinct entities, mind and body, as with two opposite qualities of all natural phenomena, and that the real antithesis lies between nature naturing itself, and nature natured; that is to say, between the responsive, selective, organizing, and in this sense creative aspect of natural phenomena, and the resulting forms of systematically organized activity retained and propagated through time, space and experience.

V. Conclusions

As briefly as possible, I have attempted to show that the principle of individuation, and the theory of related opposites to which it gives rise, can provide scientific psychology with a discipline of thought covering the universe of discourse, and promote clearly defined hypotheses for experimental investigation. The statistical treatment of quantitative studies also finds its appropriate place and function as a means of determining the degree to which any qualities assessed are in fact genuine dimensions of personality.

Permanent matter and creative mind appear to be two opposite concepts we form of one continuously evolving flow of change of which we are part. It would also seem that the

co-ordinates of introverted-extraverted awareness, intellectual organization, affective appreciation and persistent change together provide a sufficient framework of reference for a coherent description of human conduct. That they also provide the minimum number of dimensions required for the comparative study and assessment of personality is a speculative hypothesis, but one which permits of being tested experimentally.

Among the questions I have formulated, one appears to have been overlooked, namely: In any given situation why should a person choose to behave in one way rather than another? This must be the topic for a separate discussion. It may be the exception proving that the four co-ordinates I have delineated provide an adequate basis for a systematic study and description of human conduct.

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USE OF THE EEG IN THE STUDY OF IMAGERY

By P. E. BARRATT

The University of New England, N.S.W., Australia

From the Institute of Experimental Psychology, Oxford

An experiment was designed to test the hypothesis that suppression of the amplitude of the alpha rhythm provides a reliable objective index of visual imagery under two conditions of problem solution, namely, solving a 'verbal' problem and solving a 'visual' problem. Sixty-nine subjects were examined in the following way: first, preliminary 'runs' were made with each subject for EEG recording under the conditions (*a*) eyes-shut, relaxed and (*b*) eyes-open, relaxed, yielding base lines for comparison; secondly, EEG recordings were made under the conditions (*a*) concurrently with solution of the 'verbal-reasoning' problem and (*b*) concurrently with solution of the 'visual-reasoning' problem.

It was found that, although the visual content of awareness during the two problem-solution periods as reported by the subjects differed markedly, suppression of alpha rhythms occurred under both conditions, the amplitudes in both cases falling about half-way between the two values recorded in the preliminary 'runs'. There was, however, a greater suppression effect in the case of the 'visual' problem. The results, as a whole, therefore, suggest that the hypothesis is not tenable, since 'imagery' appears to be only one of many factors that may produce suppression effects. This conclusion is supported by a breakdown of the subject sample, according to reports of imagery on the verbal problem, which showed that there was no difference in the basic patterns as between those who visualized the verbal problem and those who did not.

Repeated solution in a check period, immediately following the solution period, made no significant difference to suppression of alpha in the verbal problem, but did so in the visual problem. The suggested interpretation follows Fisher (1916) in regarding this as a possible decrease in image content.

I. Introduction

Many attempts have been made to discover objective means of assessing imagery, always with discouraging results (cf. Angell, 1910; Fernald, 1912; Woodworth, 1939). None has been able to free itself from an ultimate appeal to introspection ('verbal report') as a validating criterion. This is true of a recent procedure, originally suggested by Golla, Hutton & Grey Walter (1943), and followed up by Short (1953). Still more recently (Short & Grey Walter, 1954), an attempt has been made to use performance measures, though here the precise relationship to imagery seems doubtful. The hypothesis propounded is that a relationship exists between the alpha rhythm of the EEG and the mental process of visualizing. Short's finding purports to demonstrate that the suppression of the alpha rhythm provides an objective index of visual imagery, and it is this claim that the author has set out to examine further. The critical passage in Short's paper is that which reads: 'It was found (*a*) that of the two main imagery types, visualists breathed regularly and showed frequent blocking of alpha rhythms, both of these physiological characteristics being associated with actual visualizing. Verbalists, on the other hand, breathed irregularly with alpha persistences, both characteristics being associated with verbal-motor imagery. As a rule, when verbalists attempted a problem requiring predominantly visual images, then physiological changes occurred characteristic of visualists' (pp. 50–1). It would appear to follow from this statement that the suppression of alpha rhythms provides an objective indicator of the process of visualizing, i.e. the visualizing subject reveals suppressed alpha rhythms whereas the verbalizing subject does not. Doubt has

already been cast on this claim (Drever, 1955) following a study of the EEG patterns of early blind, late blind and sighted subjects.

It would seem that a reliable objective indicator must satisfy the conditions:

(i) that its operation (variation) should be specific to the mental process it is said to indicate;

(ii) that it should occur under all conditions in which the said process is present, and should not occur in those conditions in which the process is absent; and

(iii) that, failing the above, the particular conditions under which it does operate as an indicator should be precisely specified.

Therefore, if suppression of the alpha rhythm is to be regarded as a valid objective criterion of visual imaging, it should occur whenever the subject is visualizing and only when he is visualizing. Or, if other factors enter as influences they must be specified and amenable to experimental control. Otherwise it will not be possible to examine an EEG record and conclude, from the recorded behaviour of the alpha rhythm, that, at any point, the subject must have been indulging in visual images. Short does not appear to be in opposition to this argument when he observes that verbalists behaved physiologically like visualists on visual imagery-provoking tasks.

But why introduce the notion of imagery types at all? It is at best a confused notion which can throw little light on the problem. Little can be gained from an endeavour to relate a precisely observable and recordable physiological variable with some hypothetical type classification based on 'a tendency to' or 'a habitual mode of' doing something. Surely the important consideration, in the establishment of functional relations, is to ask, regardless of supposed habitual tendencies, what the subject is actually doing in this specific situation. If he is visualizing in this particular case he is a visualizer for the purpose of the investigation. If he is verbalizing he is a verbalizer with respect to the study of the particular situation, and whether, habitually or as a general rule in a wide variety of situations, he is or is not one of these things seems to be irrelevant. The significance of this point is brought out clearly in Short's own findings where, unless the figures are misinterpreted, of a total of 384 responses made by the so-called 'verbalists,' 196, or 51% are placed in the category of visual imagery. The classification of individuals into imagery types on the basis of the pooled verbal reports from a variety of tasks, including some problems and a number of free imagination situations, and then using this as a dichotomy against which to align a physiological phenomenon associated with a particular task is a dubious procedure.

Many years ago Watt (1906) made the point that the kind of imagery utilized in thinking was a function of the kind of task. This contention of Watt has suggested a method of approach to the present problem which rejects the 'imagery-type' notion in favour of the 'specific-task' criterion. Ideally, one task which necessitated visualizing and another which eliminated it would yield data from which the relation between alpha suppression and visualizing could be tested in a crucial fashion. Such an ideal cannot be attained, yet it is possible, from the multitude of imagery studies, to provide kinds of tasks that are likely to encourage visualizing and others that do not, at least to the same extent.

It was decided, for the purpose of the present experiment, to restrict the number of tasks to two only, the one a 'verbal-reasoning' problem likely to encourage verbal-motor behaviour rather than visual imagery; the other designed to force the subject into visualizing. Verbal reports were called for, not for the purpose of obtaining an over-all imagery-

type classification, but simply to establish an 'imagery-present' or 'imagery-absent' response in the particular task, on the subliminal versus supraliminal psychophysical analogy. In the long run, perhaps, there is methodologically little to choose between the immediate report 'Imagery present' or 'Imagery absent' in a problem-solving experiment and the report 'Heavier' or 'Lighter' in a weight-lifting experiment. If the image is subliminal it may be regarded as 'not there' for the subject in the same sense as a subliminal illumination is 'not there', provided that the procedure allows for a report of it if it is there without suggesting that it ought to be. It was further decided that, as the amplitude and the degree-of-suppression of the alpha rhythm varied from subject to subject, crude measurements should be taken from the tracing of an automatic wave analyser, to be treated as ordinary individual-difference phenomena rather than placing the record in a 'responsive' or 'non-responsive' category.

II. The experimental procedure

The present investigation set out to test whether the hypothesis *that the suppression of alpha rhythms is an objective indicator of visualizing* is tenable under the condition of problem solving. The tasks were such as to demand a definite answer to a specific question. It was hoped in the selection of tasks to vindicate Watt's contention that the type of imagery used is a function of the task.

(i) *The subjects*

There were fifty-one subjects. All were male, adult volunteers from University Colleges and senior schools in Oxford. Ages ranged from 17 years 4 months to 31 years 6 months ($\bar{x}=21{\cdot}7$, $\sigma=3{\cdot}24$).

(ii) *The tasks*

There were two tasks only, which will be referred to as (*a*) the Verbal Problem (Problem Condition 1) and (*b*) the Visual Problem (Problem Condition 2), without suggesting that the subject is restricted by the nature of the task to single mental processes of verbalizing or visualizing. It was essential that the problems be presented to the subjects in such a way that they could be solved with the eyes closed, so that EEG recordings could be made at the same time. This was achieved by dividing the task into two phases, (*a*) the acquisition of the essential information and (*b*) arriving at an answer to the problem question. The subject was therefore allowed to acquire the information on which the problem was to be set before any recording was made. Then, with the subject's eyes closed, the recording started as the problem question was asked. The information was presented on cards which the subject was permitted to examine until he was able to repeat the contents.

The Verbal Problem was a modified item from a well-known reasoning test, presented as follows:

Information: (1) Digby is *five* days *younger* than Browne.
(2) Digby was born on 28 December.
(3) Christmas day is on *Friday*.

Question: 'On what day of the week is Browne's birthday?'

The Visual Problem was a figure-construction task, presented as follows:
Card I. Learn this sequence: B_3, C_2, C_5, E_3, B_3.

Card II. Examine this pattern of dots and notice how they are labelled:

A_1	A_2	A_3	A_4	A_5
B_1	B_2	B_3	B_4	B_5
C_1	C_2	C_3	C_4	C_5
D_1	D_2	D_3	D_4	D_5
E_1	E_2	E_3	E_4	E_5

Question: 'Starting from the point B_3, the first point in the sequence you learned, if you draw a straight line to the second point in the sequence, then from there to the third point and so on to the end of the sequence, how many triangles would be formed?'

It was believed that these tasks would be sufficiently unlike in the type of solution processes evoked to throw light on the relation between alpha suppression and visualizing, since, as far as the experimenter could see, the second task was extremely likely to involve visual imaging, whilst the first task was more likely to require verbal-motor processes of counting, reciting, naming and the like. This expectation was confirmed in the verbal reports as will be seen later. The problems were not intended to be difficult ones, the aim being merely to present some different kinds of problem material on which the subject could work mentally for a period of about half a minute or more in order to yield sufficient alpha rhythm data for subsequent measurement.

(iii) *Procedure*

The entire examination was conducted in the EEG department of the Warneford Hospital, Oxford, by permission of the Physician Superintendent. Two persons were present at each examination, namely, the experimenter and the hospital's electroencephalographer. On his arrival at the laboratory, the subject was seated and given a brief informal explanation of the recording procedure with an assurance that he would feel nothing. The apparatus used was the standard eight-channel Ediswan Mark II EEG and an Ediswan eight-channel automatic wave analyser. While the subject was settling down, a brief explanation of the apparatus and procedure was given, together with the information that the experimenter was interested in brain changes occurring in problem solution. Detailed instructions for the tasks, the necessity for relaxation, and the requirement to keep the eyes closed, except when otherwise ordered, were emphasized at this stage. The arrangements for presentation of the information cards and problem questioning were then explained.

As soon as the subject was clear on the details of procedure, the electrodes were fitted, one pair on the left side and one on the right in the parieto-occipital area.

The subject then lay down on his back on the couch, his head supported by pillows and, while the electrodes were connected to the input stand, he was informed of the necessity for lying very still throughout the procedure, and again reminded of the 'eyes-closed' requirement and the desirability of relaxation.

The next step was to take a trial run which consisted of a 30 sec. recording, the subject

lying relaxed with eyes closed, then 10 sec. with eyes open, and a further 20 sec. with eyes closed. The purpose of the trial run was to establish two basic measures, a maximal and a minimal for each subject, with which the problem-solving records could be compared. These two measures will be referred to as Trial Run A and Trial Run B respectively. At the end of the trial run the subject was presented with the information card for the first problem (verbal).* When he had indicated his readiness to proceed by reciting the items of information, he was told to relax and close his eyes. Then, as the experimenter asked the question 'On what day of the week was Browne's birthday?' the EEG record was commenced. When the subject gave his answer, the experimenter said, 'Keep your eyes closed, check over it and give me your answer.' After the second answer, the instruction was given 'Alright. Now relax and forget about the problem.' The recording was continued for a further $\frac{1}{2}$ min, then the subject was allowed to open his eyes. At this stage he was asked to give a report on what he had done, how he had arrived at the answer, and what went on in his mind during the work period. In the early preliminary trials on the first problem it became evident that subject after subject made no spontaneous reference to visual imagery, so it was decided to ask a question that would provide the opportunity to report visual imagery if it occurred. The question was 'Did you have any special way of representing the information to yourself to help you remember it? Did you introduce any aids to enable you to organize the material?' The procedure for the second (visual) problem was the same as the above except that two information cards were presented, one containing the letter-number combinations to be learned, the other containing the square arrangement of labelled dots. In the case of the verbal report following the solution of this problem it was unnecessary to ask the supplementary question because in every case the role of visual imagery was made quite clear in the spontaneous report.

III. Results

The ink path traced by the recording pen of the automatic analyser was taken as the source of data for the compilation of results. Taking every subject's record separately, each frequency within the accepted alpha band (8–13 c./s.) was measured for height in millimetres within each 10 sec. epoch, then averaged over the number of epochs occupied on each of the experimental 'treatments'. This procedure yielded for individual subjects four sets of values or 'scores'.

(i) Average height in millimetres of each frequency in the alpha band for recordings under the condition 'eyes-closed, relaxed, no problem'. This will be referred to as Trial Run A (Condition A).

(ii) Average height in millimetres of each frequency in the alpha band for recordings under the condition 'eyes-open, relaxed, no problem'. This will be referred to as Trial Run B (Condition B).

(iii) Average height in millimetres of each frequency in the alpha band for recordings

* Consideration was given to the desirability of counterbalancing the order of presentation of the problems. This course was rejected for the following reason. It was thought desirable to evoke a 'natural' approach to each problem, as each was designed to bring to the fore different processes of solution. There seemed, rightly or wrongly, more likelihood of 'suggestion' spreading from the 'visual' to the 'verbal' task than vice versa, because the solution of the 'visual' problem by other-than-visual means seemed most improbable, whereas the solution of the 'verbal' problem with other-than-verbal means seemed more likely, and avoidance of a 'spread-of-method' from the visual task was sought.

under the condition 'eyes-shut, solving the verbal problem'. This will be referred to as Problem Condition 1.

(iv) Average height in millimetres of each frequency in the alpha band for recordings under the condition 'eyes-shut, solving the visual problem.' This will be referred to as Problem Condition 2.

(i) *Analysis of the results as a whole*

It was decided, before proceeding to a statistical analysis of the results, to graph the average 'scores' for the fifty-one subjects for all frequencies and all four conditions, to see if there was any significant pattern which might show the result as a whole.

The outcome of this decision may be seen in the numerical form in Table 1, and graphically in Fig. 2.

Table 1. *Average height in millimetres of analyser tracing for each frequency* (8–13 c./s.) *and each experimental condition* ($n=51$)

	Alpha band (c./s.)					
Condition	8	9	10	11	12	13
Trial Run A	13·1	25·7	30·7	20·7	14·1	6·7
Problem Condition 1	9·3	17·7	21·1	15·8	10·9	5·8
Problem Condition 2	8·5	17·0	20·6	14·6	9·9	5·1
Trial Run B	7·1	9·0	10·0	7·6	7·0	4·4

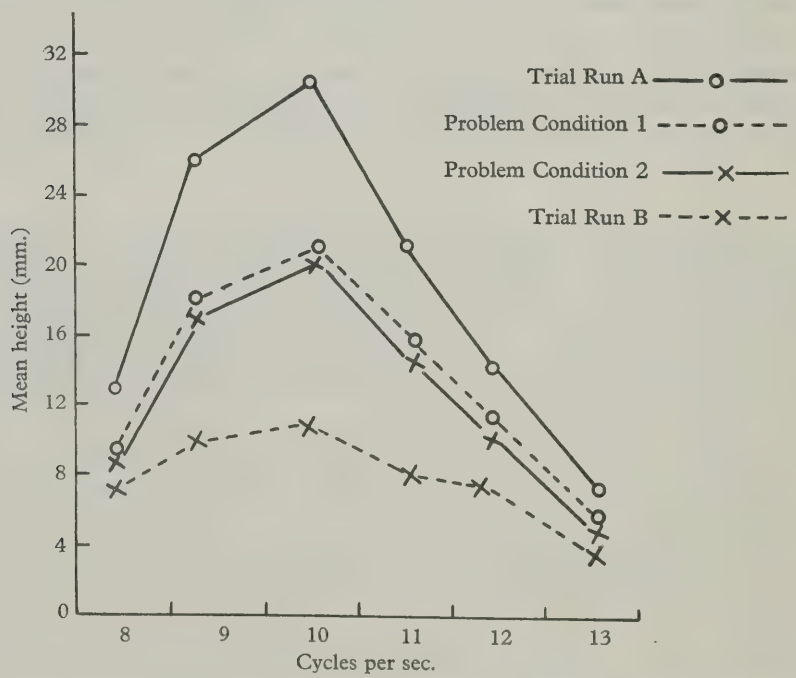

Fig. 1. Graph of average height in millimetres of analyser tracing for each frequency (8–13 c./s.) and each experimental condition. $n=51$.

A regular trend in the results for the various 'treatments' is immediately apparent. Comparing Trial Run A with Trial Run B averages, it will be observed that there is a very marked decrease in alpha amplitude at all frequencies as between the 'eyes-closed,

relaxed' condition and the 'eyes-open, relaxed' condition. It will also be observed that there is a marked decrease for both problem conditions, these following a very similar course about midway between Trial Run A and Trial Run B. The graph shows that both problem conditions influence the behaviour of the alpha rhythm in the same way.

Disregarding for the time being the subject's verbal reports, it remains to test the significance of the differences of means for the various conditions as revealed in the table and graph. This could be done for each of the frequencies separately, but it was decided for reasons of sheer computational labour-saving not to calculate significance values for each of the six frequencies in the band, but to combine the values for the three dominant frequencies in each subject's records to give a single 'score' for each individual on each of the four 'treatments'. The aim here was to bring out as clearly as possible the overall picture of alpha suppression under the different conditions. It was found in all cases that, though the particular frequency that was dominant was not regularly 10 c/.s., the dominant frequency (whatever it was) and the two frequencies adjacent to it accounted for practically all the variation in amplitude. Though there may be some doubt as to the absolute validity of the summing procedure, it seemed reasonable enough as an approximation to measurement of the total effect.

The summation procedure yielded for each subject four 'scores' corresponding to the four conditions:

(1) Average of the three selected alpha amplitude values in Trial Run A.

(2) Average of the three selected alpha amplitude values in Trial Run B.

(3) Average of the three selected alpha amplitude values under Problem Condition 1.

(4) Average of the three selected alpha amplitude values under Problem Condition 2.

Taking the distribution of these 'scores' for the fifty-one subjects under the four conditions and comparing them two at a time, it was possible to calculate values of 't' to indicate the probability that the observed divergences arose by chance. Table 2 lists the mean values for the conditions and the raw differences upon which t values are established.

Table 2. *Mean values and raw differences between means for the distributions of 'scores' obtained under four experimental conditions taken two at a time.* $n = 51$

Comparison	Mean_1	Mean_2	Difference
Trial Run A *v.* Problem Condition 1	27·637	19·902	7·735
Trial Run A *v.* Problem Condition 2	27·637	17·696	9·941
Trial Run A *v.* Trial Run B	27·637	9·196	18·441
Problem Condition 1 *v.* Problem Condition 2	19·902	17·696	2·206
Problem Condition 1 *v.* Trial Run B	19·902	9·196	10·706
Problem Condition 2 *v.* Trial Run B	17·696	9·196	8·500

Testing the significance of these obtained differences by the method advocated by Lindquist (1940) for the calculation of 't' where the measures are correlated, the values shown in Table 3 are obtained.

An examination of the tables reveals that all the obtained differences are statistically significant at a very high level. The position may be summarized as follows:

(1) There is a very significant suppression effect on alpha amplitudes in each of the two problem conditions as compared with the 'eyes-closed, relaxed' condition of Trial Run A.

(2) Both problem conditions yield significantly higher amplitude values than the 'eyes-open, relaxed' condition of Trial Run B.

(3) The difference in amount of suppression effect between Problem Condition 1 (verbal) and Problem Condition 2 (visual), though the absolute value of 't' is much smaller than in other comparisons, is still highly significant, the suppression being greater in the case of the visual problem.

Table 3. *Values of 't' and 'p' for differences of mean 'scores' obtained under the four experimental conditions taken two at a time.* $n=51$

	Trial Run A	Problem Condition 1	Problem Condition 2
Problem Condition 1	$t=7{\cdot}900$ $p<0{\cdot}001$	—	—
Problem Condition 2	$t=8{\cdot}844$ $p<0{\cdot}001$	$t=3{\cdot}745$ $p<0{\cdot}001$	—
Trial Run B	$t=9{\cdot}196$ $p<0{\cdot}001$	$t=5{\cdot}918$ $p<0{\cdot}001$	$t=5{\cdot}212$ $p<0{\cdot}001$

The statistical outcomes are not surprising when the individual subjects' 'score' patterns are examined. These show a remarkable regularity of trend with very few reversals, even as between Problem Condition 1 and Problem Condition 2 where the differences are small. The tendency throughout is for the Trial Run A value to be relatively large, the Problem Condition 1 value to be considerably smaller, the Problem Condition 2 value to be slightly smaller still, and the Trial Run B value to be again considerably smaller. The overall effect for the fifty-one subjects may be seen in the graph of Fig. 2.

Similar graphs had been drawn when $n=20$, $n=25$, $n=30$ and $n=41$, and at no stage was there any suggestion of a change in this basic pattern, thus yielding further evidence of the regularity of the trend.

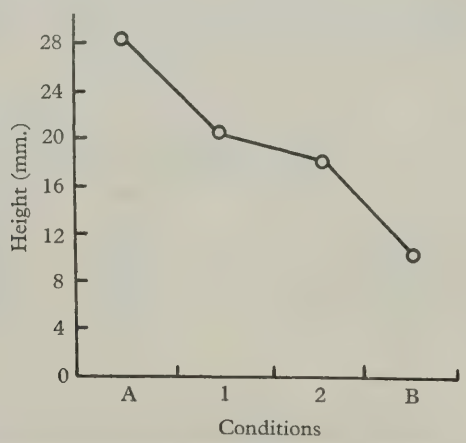

Fig. 2. Graph of the average alpha amplitudes (combining three dominant frequencies) under the four experimental conditions. $n=51$.

(ii) *Analysis for two subsamples*

So far in the presentation of results no account has been taken of the question whether the problems were in fact dependent upon different mental processes. No reference has been made to the subjects' reports recorded verbatim at the conclusion of each problem

solution. The physiological changes for the two problems have been compared just as they stand, and not without some justification as a first approximation. For, when the *spontaneous* reports for two problems are compared, a striking difference is noticeable. In the case of Problem Condition 1, there was no spontaneous report that included any reference to visual imagery. That is, without supplementary questioning, there was no evidence that visual imagery played any part in arriving at a solution to the problem. In the case of Problem Condition 2, on the other hand, every subject spontaneously referred to the part played by visual imagery in his approach to the problem. The examples quoted hereunder are typical of the kinds of verbal reports elicited by the two problems:

Subject 2

(*a*) *Verbal problem.* 'I took Christmas Day as the starting point, the 25th. The 28th is three days later and five days before that is the 23rd, i.e. two days before Christmas which gives Wednesday. Then I got confused about whether I should count back five days, or five days inclusive, hence I got two answers, Wednesday then Thursday.'

(*b*) *Visual problem.* 'I knew that the first line was going to be a diagonal, then I sort of pictured the square and drew a kind of imaginary line from B_3 to C_2, then across to C_5, then to E_3 and back to B_3. I pictured the triangles as a rather flat one on top and a larger one below.'

Subject 23

(*a*) *Verbal problem.* 'For a moment I went rather blank, then I thought Digby was born on the 28th, therefore Browne was born on the 23rd. Christmas Day was two days later and on Friday, therefore Browne was born on Wednesday.'

(*b*) *Visual problem.* 'Here I visualized the figure, not as a whole, but first from B_3 to C_2 which I could not quite see at first, then the straight line to C_5, then E_3 back down, and to B_3, a straight line up again, giving two triangles. When I checked over it I saw the lines as they occurred, making a sort of figure 8. It was fairly clear—top triangle small, bottom one bigger.'

Subject 35

(*a*) *Verbal problem.* 'I was a bit "flummoxed" at first. Then, since Christmas Day is on Friday, Digby's birthday, the 28th, is on Monday, and as Browne was born five days before that, it must be Wednesday. Then I realized that I could have done it a different way—as Christmas Day is three days before Digby's birthday it must be two days after Browne's which must be Wednesday.'

(*b*) *Visual problem.* 'Well, I got the position of B_3 and the position of C_2, then I saw that it went along from there in a straight line, then it went down, then straight up again. When I saw that the last line went straight up, I saw that it formed two triangles. I sort of saw the thing grow as I traced it out, and then it fell into a pattern.'

There is little need to add further examples. All follow similar lines and the difference between the two types of reports is obvious. At the same time it was deemed necessary to go beyond the spontaneous report in the case of Problem Condition 1 and give subjects the opportunity to report visual images if they were present. For this purpose the supplementary questioning already referred to was included after each spontaneous report and, in ten cases out of the fifty-one, the questioning elicited a reference to the presence of visual images. Some representative examples of the two types of answer given to this question are quoted below.

Verbal problem—images reported

Subject 10. 'I just remembered them successively as I wanted them and there was some visualization, e.g. when I was working back from Friday. I seemed to see the word "Thursday". I also saw "28th", not as on the card, but detached.'

Subject 15. 'I simply calculated, working from my birthday, but I pictured two men, one Digby and one Browne.'

Subject 16. 'I tried to get a pictorial method of arranging the information, remembering it as it was printed, but it seemed to get away from me.'

Subject 25. 'I tried to visualize the material as entries in a diary and could see the facts entered—a strong visual impression of the facts.'

Verbal problem—images not reported

Subject 11. 'Just seemed to keep the information in mind—condensed to the essential things.'

Subject 19. 'I learned it by heart, remembered the "word formula" then let it go, bringing it back as I required it.'

Subject 35. 'I just seemed to put the facts in the back of my mind and called on them as required.'

Subject 40. 'No special form of representing the facts. I just kept the 28th in mind, the facts just seemed to stay there, and I counted silently on my fingers actually.'

There is, then, a division within the subject sample with respect to the verbal reports on Problem 1. Ten subjects reported the presence of visual images and forty-one did not. Whilst ten is scarcely a large enough number on which to base a reliable conclusion, separate analyses of the two subsamples were made to see if there was any marked difference in the general pattern of results as between subsamples. The patterns are represented graphically in Figs. 3 and 4.

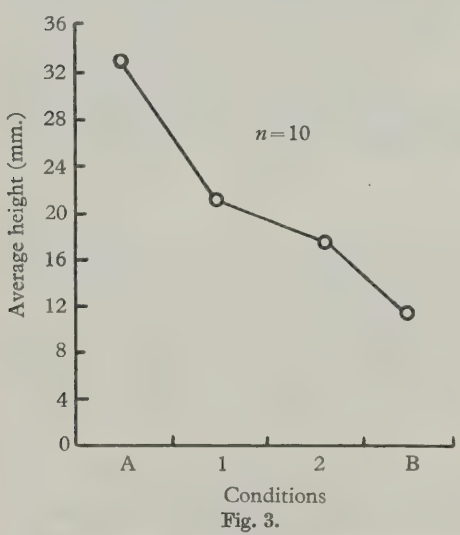

Fig. 3.

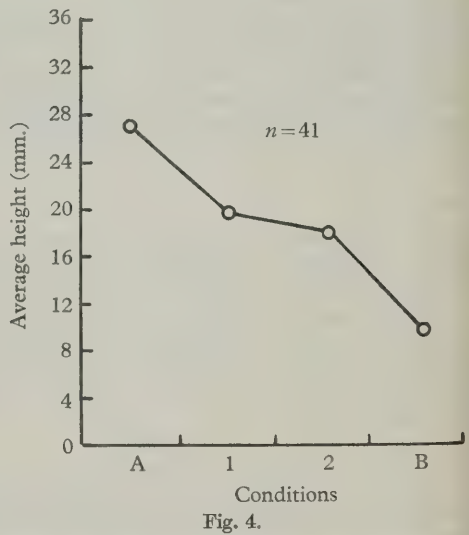

Fig. 4.

Figs. 3 and 4. Graphs of the average 'scores' of two subsamples, divided in accordance with reports *v.* non-reports of visual imagery in Problem Condition 1.

The two graphs are clearly not identical but show the same general trends, namely, a substantial drop from Trial Run A to Problem Condition 1, a further small drop to Problem Condition 2, and another substantial drop to Trial Run B. The alpha rhythms of both groups of subjects behave in much the same way except that, in general, the non-visualizing group ($n = 41$) tends to yield lower average magnitude values all round. This may or may not be significant and there is no reliable way of deciding the issue on the available data. If, however, Trial Run A is taken as a sort of 'baseline', and the amount of suppression under the various conditions is expressed as a percentage loss on this value,

it may be possible, again allowing for the probable sampling errors, to obtain information on differences between the two subsamples of subjects.

Table 4 summarizes the statistical situation, showing the percentage losses of amplitude for each subsample under each condition.

Table 4. *Average amplitude values for the two subsamples under the four experimental conditions, showing percentage losses when Trial Run A is taken as a 'baseline'*

Condition	Subsample $n=10$		Subsample $n=41$	
	Average amplitude	Percentage loss	Average amplitude	Percentage loss
Trial Run A	33·20	—	26·52	—
Problem Condition 1	21·15	36	19·60	26
Problem Condition 2	17·35	48	18·00	32
Trial Run B	11·45	66	8·65	67

Whereas the percentage loss from Trial Run A to Trial Run B is similar for the two subsamples, the reductions in the problem situations appear to be quite different. There is a greater tendency for the subsample reporting visual imagery under Problem Condition 1 to show suppression of the alpha rhythm in both problem conditions than for the larger subsample reporting no such imagery. As an offset to this finding, which would seem to support the imagery suppression hypothesis, there is the further result that the difference between Problem Condition 1 and Problem Condition 2 appears to be greater for the subsample reporting visual imagery in both problem conditions than it is for the subsample reporting no visual imagery in the first problem condition and imagery in the second, where a larger difference might be expected from the hypothesis.

Table 5. *Values of 't' for the two subsamples taken separately, showing significance of differences between mean alpha amplitudes in the four experimental conditions*

Comparison	$Mean_1$	$Mean_2$	Diff.	t	p
Subsample $n=10$					
Trial Run A *v.* Problem Condition 1	33·20	21·15	12·05	4·10	<0·01
Trial Run A *v.* Problem Condition 2	33·20	17·35	15·85	6·15	<0·001
Trial Run A *v.* Trial Run B	33·20	11·45	21·75	6·77	<0·001
Problem Condition 1 *v.* Problem Condition 2	21·15	17·35	3·75	4·88	<0·001
Problem Condition 1 *v.* Trial Run B	21·15	11·45	9·70	4·63	*c.* 0·001
Problem Condition 2 *v.* Trial Run B.	17·35	11·45	5·90	2·84	<0·02
Subsample $n=41$					
Trial Run A *v.* Problem Condition 1	26·52	19·60	6·92	7·26	<0·001
Trial Run A *v.* Problem Condition 2	26·52	18·00	8·52	7·89	<0·001
Trial Run A *v.* Trial Run B	26·52	8·65	17·87	7·72	<0·001
Problem Condition 1 *v.* Problem Condition 2	19·60	18·00	1·50	3·09	<0·01
Problem Condition 1 *v.* Trial Run B	19·60	8·65	10·95	5·00	<0·001
Problem Condition 2 *v.* Trial Run B	18·00	8·65	9·35	4·84	<0·001

The most cogent evidence to be ranged against the strict relation between visual imagery and suppression of the alpha rhythm comes from the tests of significance between the means for the various experimental conditions when applied to the two subsamples separately. An inspection of the values of 't' in Table 5 makes it clear that significant loss of alpha amplitude cannot be taken as an objective indicator of supraliminal visual imagery.

Since the suppression effect, despite variations, is significant throughout both subsamples, there is little to support an argument for alpha-amplitude loss as an objective measure of visual imagery. Both subsamples behave in very much the same fashion throughout with respect to the significance of alpha-amplitude losses in the experimental conditions.

(iii) *Comparison of values for solution period and check period*

The final check on the hypothesis rests on rather tenuous grounds and cannot, perhaps, be taken too seriously. It has been claimed by Sara Fisher (1916) and others that in thinking situations a task requiring effortful visualizing in the process of arriving at an answer often loses a good deal of its image content upon repeated solutions, as it comes to take on a more verbal formulation. Now, if one task does not, on the whole, require or involve visual imagery in its solution, it might be predicted that repeated solution, as was required in the check period of this experiment, would have no appreciable effect. If the second task does require effortful visual imagery in its original solution, it might be expected that the image content would be lessened in a subsequent solution. Therefore, if the verbal task should show no significant differences between alpha amplitudes for successive solutions whereas the visual task showed a significant gain from initial solution to check solution, the different behaviours might possibly be accounted for in terms of the suppressing effect of visual imagery, suggesting that visualizing contributes to the overall suppression effect and may in fact account for the differences between Problem Condition 1 and Problem Condition 2, though not accounting for the great bulk of the suppression effect. That something like this may have happened in the present experiment may be seen from a comparison between Problem Condition 1 and Problem Condition 2 with respect to changes occurring from solution to check period, shown in Table 6.

Table 6. *Comparison of amplitude values in solution period and check period for the two problem conditions*

	Mean value, solution period	Mean value, check period	Diff.	t	d.f.	p
Problem Condition 1	19·42	20·45	1·03	1·89	50	<0·05
Problem Condition 2	16·74	19·54	2·80	4·80	46*	<0·001

* Four subjects did not repeat the solution process.

It is evident that the gain in amplitude during the check period for Problem Condition 1 is very slight and does not reach the 5% level of significance, whereas the rise from solution to check period in the case of Problem Condition 2 is highly significant. It seems possible, therefore, that active visualizing is A factor in the suppression of alpha rhythms but not by any means THE factor. As a matter of observation verbal-motor behaviour is common to both tasks, and if the check period for Problem Condition 2 is compared with the solution plus check values for Problem Condition 1, the differences again are not significant, yet each is significantly different from Trial Run A. Table 7 shows the comparison between Problem Condition 1 (solution and check) values and Problem Condition 2 (check only) values.

All that can be said from the separate treatment of the initial solution period averages and the check period averages is that, whilst both show the same sort of suppression effects

when compared with Trial Run A as a baseline, there is a suggestion that visualizing may account for the difference between Problem Condition 1 and Problem Condition 2. This, of course, is highly speculative and requires further investigation.

Table 7. *Comparison of average values for Problem* 1 (*total solution plus check*) *and Problem* 2 (*check period only*)

Problem Condition 1 (solution plus check)		Problem Condition 2 (check only)	Difference
20·09		19·57	0·52
	$t=0{\cdot}84$ d.f. $=46$ $p>0{\cdot}10$		

IV. Discussion

All the evidence so far educed from the statistical data and verbal reports indicates that although image-provoking problem situations reduce the amplitude of the alpha rhythm, the converse proposition that amplitude reduction is a measure of visual imaging is not confirmed. Amplitude reduction is associated with visual imagery and with a number of other conditions as well; among those listed, for example, by Jasper & Cruickshank (1937) being problem solving generally, after-sensations of colour, trying to see in the dark, autonomic 'tension', sound stimulation, and tactual and pain stimulation. Stimulus input representing a 'noticeable' change, whether from external or internal sources, appears to be the general condition under which the amplitude of alpha rhythms changes. Grey Walter's (1950) speculation of the possibility that 'the spontaneous rhythms do in fact play the leading role in interpreting sense data to consciousness' has more widespread theoretical implications for psychology than the narrowing of the field to the 'measurement' of visual imagery supported in Short's paper. If the suppression of alpha amplitude were confined to visual perception and visual imagination, the discovery and confirmation of this phenomenon would have considerable, though narrow, psychological significance, since it would go a long way towards settling the problem of the relation between imagery and perception, indicating the similarity of the brain mechanisms involved in the two processes. The evidence from the present experiment and others cited in the literature points away from such a specific functional relation and suggests a mechanism of far wider significance from a theoretical viewpoint.

Returning, then, to the criteria of an objective index proposed at the beginning of this paper, it is clear that suppression of the amplitude of the alpha rhythm in EEG recordings satisfies neither the requirements of specificity, nor of agreement-and-difference demanded here. In other words, it is not possible to inspect an EEG record in a problem-solving situation and, from observation of the behaviour of the alpha rhythm, to state that the subject under examination is or is not indulging in visual imagery. Further, at the present stage of inquiry it is not possible to state the precise conditions under which the phenomenon in question could be regarded as an objective indicator of imagery. The 'other-things-being-equal' argument in this case introduces a vagueness that makes any sort of precision of statement difficult, since it cannot be known with any certainty what the variables are that have to be 'equal'.

Certainly visual perception has a marked suppression effect. Visual imaging in a problem-solving situation has a similar though less marked effect, and it is not unreasonable

to suppose that the brain events in imaging are similar to those involved in perceiving, probably activating and being acted upon by the same 'trace systems', 'neural schemata', 'reverberatory circuits' or what you will. But the phenomenon is by no means restricted to brain events associated with visual processes. It seems rather to be involved in more general situations of an 'attention-getting' or 'concentration' kind. Situations that disturb equilibrium and demand adjustment, whether the disturbance arises from external or internal sources, tend to produce the effect. There may be an important mechanism of adaptation to sensory (including imaginal and autonomic) disturbance involved, a notion that gains some support not only from the evidence already cited, but also from the 'adaptation' and 'switch-off' effects observed by Jasper & Cruickshank (1937).

The possibilities of teasing out the facts of the matter are fascinating. It could be that the suppression of alpha is another sign of tension like the G.S.R. and will resist attempts to pin it down to more definite and theoretically more interesting psychological phenomena. It remains for further researches to demonstrate whether psychologically meaningful relationships are there to be discovered.

The present study is a narrow one directed to the tenability of a particular hypothesis under specified conditions. The evidence supplied by it supports a negative finding.

The author wishes to acknowledge his debt to the British Council for the Scholarship which made the investigation possible. Thanks are also due to Prof. George Humphrey, Oxford, who supervised the project; to Dr R. G. McInnes, Physician Superintendent at the Warneford Hospital, Oxford, who made the hospital's facilities available for the research; and to Mr Douglas Lee, electroencephalographer at the Warneford, whose co-operation was a major contribution to the conduct of the investigation.

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A THEORY OF INSIGHT, REASONING AND LATENT LEARNING

By J. A. DEUTSCH

Institute of Experimental Psychology, University of Oxford

The types of performance to be explained, i.e. reasoning and latent learning, are briefly delimited. The type of structure which would exhibit such behaviour is outlined. The assumption of such a structure is then shown to explain the relevant experimental data. Some predictions are also made.

Introduction

Animals under some conditions can draw upon past experiences acquired on diverse occasions to solve a problem without trial and error. When the problem is not a novel one, we say that the animal has learnt and we point to a process of trial and error which took place when the problem was new. These situations have been dealt with in a previous paper (Deutsch, 1953). There are, however, cases in which the animal solves a novel problem without trial and error, though it draws upon information about the situation acquired on previous occasions, perhaps under conditions of different motivation and reward. These cases are called instances of latent learning and reasoning. We talk of reasoning when the animal has to combine two sets of information in some way in order to find a solution. All the information which the animal must utilize in order to solve the problem is never presented together. The animal is then set a goal which it can only reach without error by 'putting together the various pieces of knowledge'. In latent learning experiments the situation with which the animal is confronted is similar. The difference lies in the fact that all the evidence which the animal will need to use in order to reach the goal which will be set it, is presented together, sometimes in the order in which it will occur in the test and sometimes in an order determined solely by the random explorations of the animal in the maze and the random selection of points of entry and exit by the experimenter.

The word 'insight' has been variously used. It has been spoken of in connexion with such things as taking short cuts. It has also been used where an animal solves a problem without trial and error, when it has not been presented with the relevant information previously by the experimenter as in the latent learning or reasoning situation. Because of this experimental omission it has sometimes been supposed that the animal has in fact solved problems without ever having been presented with the relevant information. If we make this assumption it must be that either the animal has been so constructed that the information which it needs is already supplied by hereditary factors, in which case the label 'instinct' is appropriate, or that the solution is purely accidental. It seems, however, more plausible to believe that in these cases of 'insight' the information though not presented by the experimenter has been previously acquired piecemeal on diverse occasions (cf. Birch, 1945). If this argument is accepted, then the problem of 'insight' reduces to the same as the problem of latent learning and reasoning, defined above.

The hypothesis

A theory of learning and performance under goal-seeking conditions has already been put forward in a previous publication (Deutsch, 1953).

As it is proposed to use essentially the same system with one or two additions to account for the types of ability described above, it is necessary to give a brief résumé of the theory.

The system is made up of five elements. These elements are fully defined by the relations between themselves and other elements. Only some of them can be given a physical identification at present. These elements are called: (*a*) analyser (to be identified with a receptoral system), (*b*) a link, (*c*) a motor (or effector organization), (*d*) an environment, (*e*) a feature of the internal medium (to be identified with a hormone, etc.). These elements can all be considered as the same, differing only in the way that they are connected to the others.

The relations obtained between these five elements can be reduced to three. These are called: (1) activating, (2) switching off, (3) causing to vary.

The following are the rules of the system:

(1) Links are arranged in single rows. That is, each link has only two neighbours and the first and last only one.

(2) Some of the first links in these rows are set into activity by a feature of the internal medium in proportion with the magnitude of this feature (such as dehydration).

(3) The first link excites its neighbour (since any link which is excited, excites the next link) and so on until the whole row is excited.

(4) Each link is connected to a portion of the motor system which it excites when it is itself activated.

(5) When the row of links is active only the last link to be excited will excite its portion of the motor system fully.

(6) The excitation of the motor system causes the features in the environment to vary.

(7) A particular feature in the environment activates a particular analyser.

(8) An analyser decreases the excitation transmitted by any link to which it is attached according to the amount of firing from the analyser. This link will take some time before it resumes full irritability and so before it transmits excitation to links further in the row.

(9) Each link when it is active and exciting the motor system will persist in exciting the same part of the motor system whilst the input from the analyser attached to it increases. When there is a decrease, its motor output will vary. The link will remain connected, when it is switched off, to the particular part of the motor system which it was exciting just before this event, so that when the link is again switched on this output will be present.

These last two propositions represent complications of the original hypothesis, now perhaps warranted by experimental work conducted by the author into the relationship of sensory input to motor output. It is assumed that each analyser, such as that dealing with the reception of a particular pitch, fires to an increasing extent, the closer the correspondence to it of an incoming message. This rate of increase is picked up by a differentiating device. If the correspondence increases, the same action is further continued, if it decreases the course of action is altered. Thus when we endeavour to maintain a certain pitch in singing, we correct when we hear that we are straying away from the

set note and reverse our course of action when this last correction has begun to lead us into the opposite error and we perceive a decreasing correspondence. One of the predictions from this view is that a reversal of the sign of the error where a conflict of cues can be excluded should not lead to an impairment of the ability to reach and maintain a note (to use a previous example).

To give a concrete example, the following is assumed to occur when a thirsty animal is running a maze for a reward of water. The 'landmarks' in the maze are taken to be 'represented' by analysers arranged in the order in which the animal has found them on previous runs. The 'cues' or 'landmarks' nearest the goal are 'represented' by analysers connected to the link next in order to that excited by dehydration. Those cues at the entrance of the maze are farthest away on the row of links from this first link. When this first link is excited by dehydration, the whole row of links will be excited link by link. The last link will in all probability be switched off without any activity on the part of the animal as the experimenter sets it at the entrance of the maze. For at this juncture it will see from a distance some other cue to which it will attempt to approximate itself. During this activity further cues will come into view and as this happens the animal will locomote towards these. Any cue nearer the final goal will be selected rather than one farther away—for any analyser which is set off by a cue nearer the goal will be attached nearer the first link from which the excitation in the row is originally derived. When this analyser is set off, the link to which it is attached will cease transmitting excitation to its neighbour, and so cut it off from all the links farther away from the first link than itself. In this way a clearly visible short cut will be taken as soon as it is introduced and blind alleys will be eliminated.

The assumption is made that there exist in the nervous system of an animal prior to learning many sets of units (called links). These links are joined together in rows such that when the first link is excited it excites the second, which in turn transmits this excitation to the third, and so on.

Further, the various 'first links' are each excited by different humoral factors. It is this excitation which is passed from link to link. Each link is connected to the motor system, but will excite it when it is itself passing no excitation to the next link.

This will occur in the following way. Different analysers, or receptoral organizations are attached to each of the excited links. When these discharge they cause the link to which they are attached to switch the excitation which it is transmitting to its own control of the motor system.

These analysers are attached to the first few links innately. To the others they are connected whenever an analyser already connected to a link discharges.

(10) When an analyser already attached to a link in the row is excited the analysers which have been fired the moment before will be attached to the empty links at the end of the row. These fresh analysers which fired off nearest in time to the analysers already attached, will be fastened to the empty links nearest in order (next to) the links already occupied by analysers. Thus, temporal proximity of firing of analysers will lead to ordinal proximity of attachment on the row of links.

This attachment will not take place when the order of stimulation is already recorded in the system. Further, if an analyser attached to a link in the middle of a recorded sequence is touched off, it will be connected to an empty row of links together with the analysers which fired off prior to it (see further rule 14).

Thus the animal will retain a record of the order in which cues occurred in relation to each other and the goal. The factor leading to reinforcement, primary and secondary, is the firing off of an analyser already attached to a row of links. The theory is thus able to reconcile the apparently contradictory results of Kohn (1951) and Miller & Kessen (1952) on one side, and Sheffield, Wulff & Backer (1951) on the other (Deutsch, 1953).

However, on future occasions the firing of one analyser may not be succeeded by the firing of the next, because the environment alters.

(11) The 'threshold' of excitation of the link x, the analyser of which has fired, will rise if it is not switched off, for instance if the analyser attached to the succeeding link does not fire. This threshold will continue to rise while this switching off is delayed.

Thus when the first link in the row is again excited on a subsequent occasion the amount of excitation transmitted to the link x and from it to its neighbour and so to the rest of the row will be diminished.

(12) This 'threshold' of excitation of a link will be lowered each time a link is switched off. The lowering of the threshold will be proportionate to the intensity of excitation present in the link at the time. One of the consequences is that the more often cues arrive in the recorded sequence the more excitation will be passed through the row of links where it is again excited.

(13) If there are two or more rows of links excited, the links at the end of the rows may be setting off movements that are incompatible. In this case the link with the highest rate of excitation will determine the movements.

Hence, as a deduction from rules 12 and 13, when two links are setting off competing actions the one which has fewer links between itself and the first link will obtain precedence, other things being equal.

There will frequently arise occasions on which one analyser will be found to be fastened to two or more different rows of links. A cue will have been noticed which is common to two or more situations at once.

(14) When an analyser is connected to two or more links at once, a small fixed proportion of excitation will be passed through it from one link that may be excited to the others to which it is fixed. (These will transmit this excitation to their neighbours according to rule 3.) These links may either be constituents of the same row or other rows. The greater the number of analysers which two links or rows have attached to them in common, the greater the transfer of excitation from one to the other.

(For the sake of graphic convenience, when there are many analysers common to two links, they will be represented just as a single analyser. This analyser, according to the number it actually represents, will be spoken of as transmitting more or less excitation according to a rough criterion of similarity.) A machine embodying the main rules of the theory has been constructed (Deutsch, 1954).

Explanations

Having set up this comparatively economical system we are now in a position to examine how it would explain some of the evidence. Let us begin by taking an experiment originally performed by Tolman & Honzik (1930), and more recently repeated by Caldwell & Jones (1954), in an enclosed maze. We have Osgood's word (1953) for it that it is a case of reasoning to which 'a Hull type analysis would not be applicable'. (This was,

it is true, when it seemed that the result could not be repeated.) In the experiment of Caldwell & Jones (1954) the rat was taught three paths to the same goal, a straight alley and two other paths. One of these was much longer than the other and joined the straight alley nearer the goal than the other detour path. After training, the straight alley was blocked at a well-demarcated point before the longer path joined the straight alley but after the shorter detour had already met it. The rats after this experience of the block mostly chose the longest path, the only one which the block did not affect.

Assume that three rows of links were connected to various analysers during the process of learning the three alternative pathways. According to the hypothesis put forward, the analysers attached to a row of links, and their order, correspond to the cues picked up by the rat, and their order, in each alley. However, as reduplication of an order already laid down does not occur, those parts of each path which overlap are all represented on the same row of links. Those parts of the paths which do not overlap are arranged on separate rows (rule 10). One of these three parts will be represented on the same row as the common part of all the paths, depending on which path was learnt first. The other two parts will be organized on separate rows and connected to the row representing the path learnt first via common analysers. ('If an analyser attached to a link in the middle of a recorded sequence is touched off, it will be connected to an empty row of links together with the analysers which fired off prior to it.') When the animal finds that the straight alley to the goal is blocked, then according to rule 11 'the "threshold" of excitation of the link x, the analyser of which has fired, will rise if the analyser attached to the succeeding link does not fire and will continue to rise the longer this is delayed' (Fig. 1). Now both the straight alley and the shorter detour have that portion of the runway in common in which the block is inserted. This means that both paths are represented by the same analysers attached on the same links. Now when this sequence does not recur, there will be a decrease in the excitation which is transmitted past the point where the break in sequence occurred. Consequently, when the animal is again placed at the choice point, it will tend to go via the path which is 'represented' by that row of links in which there is no impediment to the transmission of excitation between the link 'representing' the choice point and that 'representing' the goal. For, according to rule 13, 'if there are two or more rows of links excited, the link with the highest rate of excitation will act on the motor system'. Hence the rat will choose the longest detour.

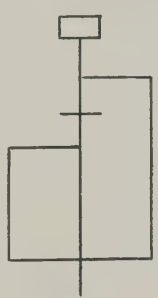

Fig. 1.

Hull (1935) has, however, attempted to explain some of Maier's (1929) experimental data on reasoning. He uses an imaginary experiment, bearing a remote resemblance to the sort of situation that Maier used. To invite comparison, the same form will be used here. Four goal boxes are used, R, U, X, and H. R is connected with every other box and U is connected to X (Fig. 2). The rat is then trained to run from R to X for a reward of food. Following this it is trained to go from U to X for food reward, from R to U for a water reward and from R to H for a water reward, in that order. We may assume that after learning the tendencies to go to U and H when thirsty on R are equal for the rat. In the crucial trial the animal is placed hungry on R with the path to X blocked. There is then only one way to X and this is through U. The rat has never been this way before in any one trial. However, it appears plausible that rats would synthesize their partial experiences. They would run towards U, and then to X, more frequently than to H.

Assume that four rows of links have been occupied by analysers during training, each consisting of four links. (This number is arbitrary and adopted for the sake of simplicity.) These may be represented as shown in Fig. 3. The first link has an analyser which responds to food attached to its first link, then an analyser set off by a feature on platform *X*. There follows *UX*, an analyser set off by a landmark between *U* and *X*, and then *U*, an analyser 'representing' platform *U*. The other rows in the diagram are to be similarly interpreted.

When the animal is made hungry, rows 1 and 2 will be excited according to rules 2 and 3 (rule 2: 'The first links in these rows are set into activity by a feature of the internal medium in proportion with the magnitude of this feature (such as dehydration)'; and rule 3: 'The first link excites its neighbour (since any link which is excited, excites the next link) and so on until the whole row is excited'). This excitation will be transmitted to row 3 via the common analyser *U* and to row 4 via water analyser. (Rule 14: 'When an analyser is connected to two or more links at once, a small fixed proportion of excitation

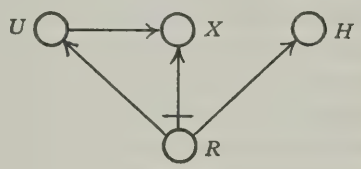

Fig. 2.

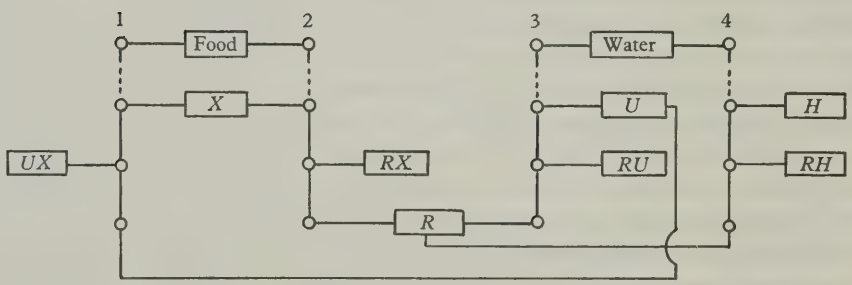

Fig. 3.

will be passed through it from one link that may be excited to the others to which it is fixed.') We may neglect for the purpose of this explanation the excitation transmitted via the analyser *R* to rows 3 and 4 as the excitation in the link to which analyser *R* is attached in row 2 will cease as soon as *RX*, a cue between *R* and *X*, appears (rule 8), when the animal is placed on the platform *R*, assuming that *RX* is in front of the block. The amounts of excitation at the links to which analysers *RU* and *RX* are attached are unequal, as more links intervene between *RU* and the source of excitation than *RX*, and *RH* than *RU* (rules 12 and 13). The animal will accordingly approach *RX* (rule 13). As there is a delay in the appearance of *X* owing to the block, excitation at the link to which the analyser *RX* is attached will decrease (rule 11) and the link to which analyser *RU* is attached will begin to determine output. Hence the animal will take the path leading to *U* and thence to *X*.

From a comparison of this explanation with that of the experiment on p. 122 it will be seen that success in solving the problem should only occur if there is a sufficiently

large difference in the number of links between *U* and *RU* on one hand and *U* and *RH* on the other. Translated into experimental terms the platforms should be large, and well furnished with many cues, so that the 'psychological' distance between the water cups and the two alleys is made large. Unless this condition is met, success in Hull's 'conventionalized version' would not be expected to occur.

Unfortunately this prediction does not differentiate between this theory and Hull's. Hull's explanation of his own fiction is fallacious.

It should be clear from the preceding analysis that the hypothesis should explain Blodgett-type latent learning. Here the rat is allowed to explore a maze without a reward. It is then rewarded in one place in the maze and then inserted in it at a different point. The rat can then apparently find its way to the place of reward, perhaps without an error.

The rat, according to the hypothesis, should in its random explorations record the order of cues preceding something with which it was familiar (that is, a cue which 'represented' an analyser already attached to a row). Some of the cues would be noticed

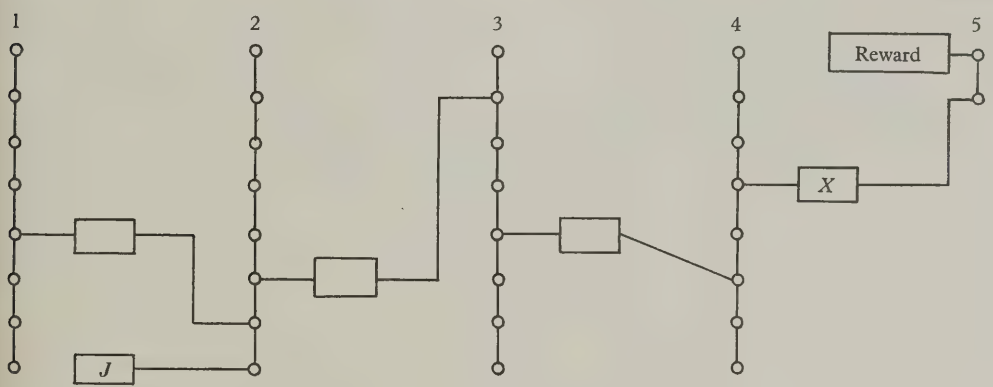

Fig. 4.

again on subsequent explorations. When the animal is motivated and given a reward in the proximity of a distinctive cue, the excitation due to this motivation will flow through the analyser to any other rows of links to which it is attached and from these to any further rows of links through analysers which they have in common. The animal will then find its way to the reward from any point in the maze, providing that the cues which it picks up where it is placed are represented by analysers connected to links which are indirectly influenced by excitation emanating from the link to which the reward analyser is attached. This may be seen by inspecting Fig. 4. (Only common analysers are indicated.) Suppose that excitation is being introduced at *X* from row 5. All the rows 1–4 will be excited. Also suppose that the animal is so placed in the maze that it is receiving stimulation from an analyser attached at *J*. The analyser attached to the next link on row 2 will then fire, switching off the excitation both to the link to which *J* is attached and to row 1. When, while approximating itself to the cue which had set off this analyser, it picks up signals from the next, row 2 will also be switched off as also the initial part of row 3. Further approximation to cues represented by analysers attached to links still excited can only bring the animal nearer the source of excitation, and so to the reward. The mechanism would operate in the same favourable way if the animal were placed at

X with J as the goal. If excitation is introduced into a row via two different analysers, and this gives rise to conflict, it will be resolved by the operation of rules 12 and 13.

From the foregoing it would appear that the animal should find its way to any reward which it noticed, whatever its motivational state. This, however, is a superficial deduction. Though the information may have, in fact, been registered, it may not in some situations lead towards a correct performance. For instance, many experiments are done in such a way that a prominent identical cue is found in both the goal boxes between which the animal must choose on the critical trial (Kendler & Mencher, 1948; Grice, 1948). An animal is made hungry and then run to two goal boxes containing food. In one of these water is also placed. Then the animal is made thirsty. It fails to choose the relevant side. Fig. 5 shows a schematic representation of the hypothetical mechanism in the animal at the time of the choice in the critical trial. Excitation originating in row 3 owing to dehydration is conveyed to row 2 by the common analyser. Row 2 is excited and this excitation transferred to row 1 via the analyser common to rows 1 and 2. As there has been an equal amount of reward and practice on rows 1 and 2, the only thing

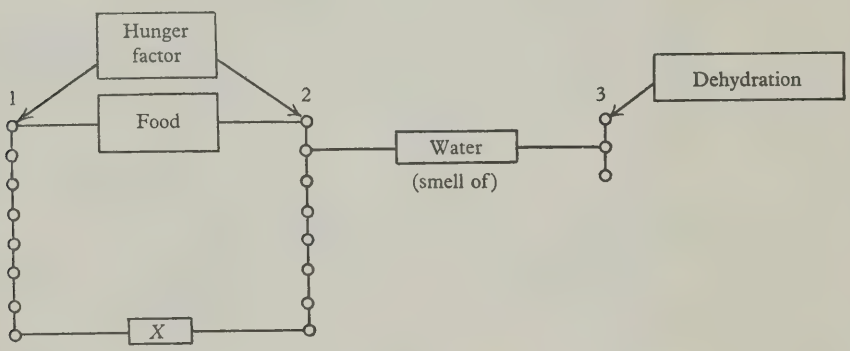

Fig. 5.

which could decide that row 2 should control output is the slight disparity in the number of links (and analysers) through which the excitation has to travel in rows 1 and 2. It is assumed that such a difference is too small to eventuate in the correct performance.

Such an explanation of the failure to find latent learning in the above situation may not seem plausible. But it can be checked in one or two ways. For instance, if the water analyser was attached farther from the beginning of row 2, there would be a greater disparity between the number of links between analyser X on rows 1 and 2, with the balance more heavily in favour of row 2. Such experiments (with food as the irrelevant incentive) have in fact been done, one by Thistlethwaite (1952) and the other by McAllister (1952), with the result deducible from the present theory. They found that rats showed a significant tendency to choose the relevant side when they were made hungry if the relevant reward (water) was widely separated spatially from the irrelevant reward (food) in one of the arms of the maze. The difference between the results of Spence, Bergmann & Lippitt (1950), and Maltzman (1950) also supports this explanation.

There is another way of testing the hypothesis in its application to the present instance. That is to have two differing relevant incentives in the two arms of the maze with the irrelevant incentive only associated with one. There would thus be no common analyser to transmit excitation from row 2 to row 1, in the way the 'food' analyser does in the

diagram. The animal should then make the correct choice. The argument in the above deductions rests on the assumption that there is only a relatively small number of other common analysers in the two arms of the maze past the choice-point (except those associated with the reward in some instances). The argument rests on a ratio of analyser to link resistance ratio. Obviously the greater the number of common features in the two alleys the less likely the animal is to choose the relevant path. Hence the animal's success will depend on where in the sequence it picked up similar cues (which is usually left to chance, but which could be varied) and hence on the dissimilarity between the two alternative paths.

Another type of experiment where animals have failed to show any evidence of latent learning is where the relevant reward is placed in one goal box and the irrelevant reward in another (Spence & Lippitt, 1946; Kendler, 1947*a*). If the animal is made hungry and shown water in one alley and then rewarded with food in the other, it will still tend to go towards the food side when it is made thirsty. Assuming there are some similar cues common to both alleys, a portion of the 'thirst' excitation arriving at the row of links 'representing' the alley leading to the incentive originally used as reward will be transmitted by a set of links with much lower thresholds. For according to rule 12, the combination of high excitation and reward in this row should have reduced the 'resistance' to the passage of excitation considerably in this row. Thus, though it receives less excitation to begin with, any excitation passed on by it should receive less 'attenuation'. Hence the animal should either behave randomly, fail or succeed according to the intensity of motivation on the rewarded food trials, the number of times it was food-rewarded and the similarity of cues on either side. Increasing the water motivation on the critical trial should make no difference to its choice. If there had been an equal (or zero) amount of reward on both sides, the animal should choose correctly unless there were strongly similar cues along the two different paths (Kendler, 1947*b*, Meehl & McCorquordale, 1948). Strong irrelevant motivation should not in itself hamper the subsequent utilization of information provided the motivation has not been rewarded. This deduction is borne out by Strange's (1950) experiment. Animals which had been deprived of water for 22 hr. were given an equated amount of training to two goal boxes in a U maze. One goal box was empty, the other contained food. When they were made hungry and allowed to choose, they gave evidence of having learnt the location of the food.

In case it is felt that the hypothesis presented is entirely *a posteriori*, here is a rather curious prediction. Assume there are two rows, both equated for 'resistance'. Further assume that they both share an analyser *X*, but that this analyser is in different ordinal positions on each (Fig. 6). Then if we excite both rows simultaneously and equally the animal, being given a free choice, should choose the path which contained the cue corresponding to analyser *X* nearer to the choice-point, though invisible from it. Such an experiment could be performed in the same sort of conditions as a spontaneous alternation experiment.

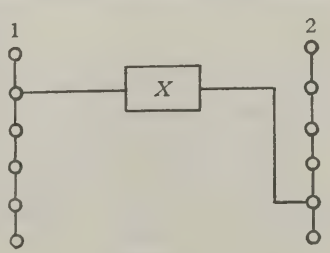

Fig. 6.

The hypothesis put forward is, however, exposed to numerous objections. It will be said that it would predict no learning in a situation where the animal has to take a

different path to the same goal box according to its motivational condition (though learning to go to different goal boxes should be rapid). That animals find the second easy and the first inordinately difficult is indeed the case, but we cannot equate slowness with complete inability. The relevant experiments have been done by Hull (1933) and Leeper (1935). However, there is a possible way of escape for the theory as we assume that the excitation travelling from one row to the other is not transmitted in its entirety, and that the proportion transferred remains fixed. The proportion of excitation transmitted directly from link to link would be expected to rise after each rewarded trial. There would thus be a progressive increase of the absolute difference between the competing links (at the choice-point), and slow learning. On this view the speed of appearance of the correct response would be a function of the similarity of the goal boxes, if we were using two.

It is doubtful, however, whether this explanation could cover all forms of conditional learning. Neither will it account for many other phenomena customarily included under the heading of higher mental processes such as delayed reaction, double alternation, abstraction and perception of relations. However, it would be possible to extend the hypothesis or to put forward a set of complementary hypotheses. This has already been done to deal with a special case of relational perception (Deutsch, 1955). It is, however, unwise to embark on various speculative elaborations to cover remote and complex cases before the more basic ideas in the theory have been tested and verified to some degree. The present modification and extension of the previous theory was prompted by the fact that one of its important predictions has now been confirmed in a study (as yet unpublished) by N. S. Sutherland. The prediction is that spontaneous alternation is goal alternation as well as stimulus alternation. Hence, if an animal is equally rewarded and trained on two sides of a T-maze where the goal boxes are out of sight at the choice-point, then if it is directly placed and fed in one goal box, it should tend to run to the other when it is placed again at the entrance of the maze and given a free choice. This tends to confirm the notion, unique to the theory, that 'motivational' excitation is transmitted through units underlying acts nearer the goal to those farther away from it, and that stimulation, far from increasing this excitation, actually severs its flow.

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SEX DIFFERENCES IN PREFERENCES FOR SHAPES: A CROSS-CULTURAL REPLICATION

By G. JAHODA

University College of the Gold Coast

A shape-preference test was designed by McElroy on the basis of 'a possible corollary of psychoanalytic theory' and administered to Scottish schoolchildren. In accordance with predictions there was a significant tendency, increasing with age, for boys to prefer round shapes (disguised female symbols) and for girls to prefer rectilinear ones (disguised male symbols). McElroy suggested alternative hypotheses that might, in his view, account equally well for the effects obtained.

In the present study McElroy's main alternative hypotheses were examined and it appeared that, as formulated by him, they could not be used to predict results in Accra. It was shown, however, that the psychoanalytic hypothesis did permit a definite prediction: in a cultural situation where there is less repression, one would expect sex differences to be smaller. The test was given in a modified form, which made more detailed analysis possible. A greatly reduced, but still significant, sex difference was in fact obtained. Age changes, contrasting with those found in Scotland, were also shown to be consistent with the psychoanalytic hypothesis. It was further suggested that some of the specific Freudian interpretations of symbols may have to be reconsidered.

I. Cultural contrasts

In a recent paper, to which reference should be made for full details, McElroy (1954) described a shape-preference test and reported the results of its administration to Scottish schoolchildren. He discussed several hypotheses considered relevant in connexion with an obtained sex difference. The aim of the present study was to discover if such a sex difference exists in a very different cultural setting; and if this was the case, to test some of the hypotheses advanced.

At the outset a brief sketch of those cultural factors that appear relevant in the present context will be attempted. The work was carried out in the Accra region where the Ga tribe, to which most of the sample belongs, is concentrated. Their traditional family structure is of a form rarely encountered in any other part of the world. Men and women live in separate households. The wife or wives in turn supply the husband with food and visit him in his room at night. Children below puberty live in the women's compound. At puberty boys move to their father, whilst girls remain with their mother until they are married. This pattern is gradually changing, but there is evidence that it still holds for a considerable proportion of the population.

The writer has occasionally observed sex-play among small children; adults who discover it happening will discourage this, but without any obvious signs of disgust and moral indignation. Although in the past pre-marital chastity was apparently stressed, it is the exception rather than the rule to-day.* The sterner rules of Christian sexual morality are not very firmly entrenched, as is evident from the large numbers of nominal Christians

* Field (1940), the chief authority on the Ga people, wrote as follows (p. 39): 'If the bridal night proves t the husband that the bride was a virgin he takes to her parents, on the following morning, a piece of new calic stained with the blood of the ruptured hymen. If he finds that she is not a virgin he has the right to demand c her parents a goat to be slaughtered to stain the calico. But in recent years this custom has almost died out for, I am told, "not one girl in a thousand is a virgin". Schoolgirls are said to be the least chaste of all an frequently sell their favours for small sums of money.' There is no doubt that the social changes brought by th War, especially the presence of foreign servicemen, have greatly enhanced the trend in this direction.

who enter polygynous marriages. One may say with confidence, therefore, that there is likely to be far less repression among the Ga than in Scotland.

In connexion with McElroy's 'stimulus generalization' hypothesis it is also worth noting that boys have extensive opportunities of seeing the exposed breasts of girls and mature women.

Turning to another relevant topic it can be stated that there are hardly any indigenous ornaments, except perhaps on pottery, and these are invariably round; the same is largely true of the decorations on imported cloths. In general, few Gold Coast Africans appear to be interested in pictures as such, apart from a small Westernized minority. In most ordinary African houses in Accra one can usually find photographs of family members or portraits cut from magazines, but hardly ever representations of any other objects.

As against this some Western influences, acting chiefly upon schoolchildren, must be considered. First of all 'art' is taught at schools; shields are extensively used as sports trophies in both boys' and girls' schools; and lastly there are several flourishing Scout troops in Accra, so that the Scout emblem is fairly well known among children and young people.

II. McElroy's hypotheses and possible predictions

Looking at McElroy's main hypotheses in the light of the above, it becomes apparent that they are of very unequal value. In particular, he claims that 'the well-established principle of stimulus generalization' could have been used to predict the sex differences. A plausible contention at first sight, but it will not bear closer examination.

Examples are quoted of generalization of fear responses exhibited by rats 'from one food cup to a highly similar drinking cup', and by cats from one particular food-box to 'all food-boxes and associated configurations'*; then, without any transition, follows a reference to 'associated configurations of the male and female form'. This seems rather an unwarranted jump, considering among other things the circumscribed nature of the experimental work on stimulus generalization, and the findings concerning generalization gradients; these show a progressive decline of response as the degree of similarity between the stimulus objects decreases (cf. Hovland, 1951; Vinacke, 1952). The phrase 'associated configurations of the male and female form' disguises the fact that the common elements shared by women and curved drawings on the one hand, and men and rectilinear drawings on the other, are severely limited. Furthermore, the attempted explanation of age-changes in preferences by means of this hypothesis as 'generalization of fear responses' connected with 'female adolescent development' is too patently *ad hoc* to be very convincing.

In the present case the hypothesis did not seem to permit an unambiguous prediction. With reference to the boys it might be argued that the more frequent exposure to the primary stimulus (i.e. female breasts) would lead to wider generalization and therefore to a greater sex difference than that found in Scotland; but it is equally possible that 'negative adaptation' will occur, or in other words that the boys will become accustomed to and blasé about the sight of female bosoms, with a consequent lessening of generalization; if the latter holds, the sex difference should turn out smaller than the Scottish one. It will be seen that the formulation is so loose that no clear-cut consequences can be deduced, and a hypothesis which is incapable of being falsified is worthless.

* A somewhat more relevant example might have been Watson and Rayner's (1920) experiment; it will be remembered that they conditioned a child to fear a rabbit and found a generalized fear of dogs, fur coats and Santa Claus masks.

A similar criticism could be made of McElroy's references to identification or 'empathy'. He says (p. 212): 'Considering the direction of the deviation, it is apparent that identification responses have not provided the principal basis for preference, although it is likely that they have contributed towards the total experimental results.' No evidence whatever is offered to show that identification did in fact operate.

In contrast with this the psychoanalytic hypothesis can be made the basis for a definite prediction; there is less repression among the Ga, hence sex differences will be less pronounced than in Scotland.

III. Sample and test administration

The main experimental group consisted of 858 boys and girls from eight different Middle Schools in Accra. These schools are approximately equivalent to the former Senior Elementary schools in Britain. In each the top and bottom forms were tested in order to obtain an adequate age spread. In the absence of compulsory registration reliable information about ages is impossible to obtain. The pupils' own estimates were therefore accepted; although subject to considerable error in individual cases they do make it possible for the sample as a whole to be at least roughly divided into an older and a younger group. For

Table 1

	Males		Females		Proportions of preferences: 'Female' shapes		Proportions of preferences: 'Male' shapes			
Item	*A*	*B*	*A*	*B*	Male	Female	Male	Female	χ^2	*c*
1	153	275	137	293	0·643	0·681	0·357	0·319	1·35	1·16
2	189	239	101	329	0·442	0·235	0·558	0·765	36·93	6·08
3	341	87	300	130	0·797	0·698	0·203	0·302	8·22	2·87
4	269	159	235	195	0·629	0·547	0·371	0·453	5·71	2·39
5	240	188	254	176	0·561	0·591	0·439	0·409	0·79	0·89
6	336	92	266	164	0·785	0·619	0·215	0·381	23·50	4·85
7	224	204	221	209	0·477	0·486	0·523	0·514	0·04	0·20
8	96	332	145	285	0·776	0·663	0·224	0·337	10·74	3·28
9	79	349	94	336	0·815	0·781	0·185	0·219	0·91	0·95
10	91	337	83	347	0·787	0·807	0·213	0·193	0·19	0·44
11	63	365	83	347	0·853	0·807	0·147	0·193	1·68	1·30
12	194	234	222	208	0·453	0·516	0·547	0·484	3·40	1·84

$\chi^2 = 23{\cdot}89$; $r_p = 0{\cdot}05$; $c_{mf} = 4{\cdot}89$.

both boys and girls the modal age estimate in Form I was 13 years and in Form IV 16 years, with a total range from 11 to 19 years.

Classrooms are often greatly overcrowded, so that a certain amount of copying cannot be prevented; no mixed classes were therefore included in the sample.

For the subsidiary experiment two further schools, one boys' and one girls', were used. As the age distribution did not matter in this case, pupils in all four forms were included, totalling 278 boys and girls.

The actual drawings were taken over from McElroy by careful tracing, with only one minor modification: the horizontal bar across the lower part of the fleur-de-lys was left out in order to avoid too specific a representation of the Boy Scouts' badge. The general layout, however, was altered in an attempt to secure data which would permit the checking of some of his hypotheses. All pairs of drawings were arranged in a single column on the centre of a foolscap sheet in such a way that rectangular blank spaces were left on either side of each pair of drawings.

The first part of the instructions was closely similar to those given by McElroy, except that the subjects were asked to put a ring around the picture they preferred and to cross out the one they liked less; this seemed the safest way of avoiding misunderstandings due to language difficulties. When everybody in the class had completed recording their preferences, they were invited to write by the side of each picture of what it reminded them.

IV. Results of main test

The preferences of boys and girls for 'male' and 'female' shapes are shown in Table 1, laid out in exactly the same way as McElroy's table with the Scottish results and computed on the same basis.*

Significant sex differences emerged for only five out of the twelve items; one of these, no. 4, happens to be the single non-significant item for the Scottish sample. The pooled results of all items produced a χ^2 of 23·89, corresponding to a critical ratio c_{mf} of 4·89. This represents a rather low level of significance in comparison with the Scottish results, where a χ^2 of 154·56 was obtained, which is equivalent to a c_{mf} of 12·43. The overall fourfold point correlation r_p was 0·13 in Scotland and 0·05 in Accra.

The mean male test 'score' was 8·00 preferences for disguised 'female' configurations or 4·00 preferences for disguised 'male' configurations; the standard deviation of the male 'scores' was 1·72 and the standard error 0·083. The mean female test 'score' was 4·57 preferences for disguised 'male' configurations or 7·43 preferences for disguised 'female' configurations; the standard deviation of the female test 'scores' was 1·79, and the standard error of the mean 0·086.

The relationship between age and preferences was again analysed by McElroy's method. For reasons explained above the age division had to be shifted upwards by two years. The number of preferences for 'female' shapes are given below:

	Male		Female	
	No. of subjects	No. of preferences	No. of subjects	No. of preferences
Below 14 years	104	820	114	765
14 years or above	324	2603	316	2428
	$c_d = 0{\cdot}81$		$c_d = 5{\cdot}29$	

There is a striking contrast here with McElroy's findings; whilst the Scottish boys and girls both diverged significantly with rising age, the Accra boys did not change and the Accra girls converged significantly towards the boys' preference pattern.

* There appear to be several computing errors in McElroy's numerical results. None of them is large enough to affect his general argument, but for the purpose of comparison corrected figures will be used.

First, he arrived at the overall χ^2 value of 169·85 by adding up the individual χ-squares for item nos. 1–12; this is quite in order, provided the degrees of freedom are also added up; but he proceeds to use the square root of χ^2 thus obtained as an estimate of the critical ratio, which can only be done with one degree of freedom.

The writer did not work out the fourfold point correlations for each item, as they convey hardly any additional information. The r_p for all items pooled was computed from McElroy's data as being 0·13; it is not clear how he arrived at an 'average' figure of 0·14.

Finally, the standard errors of the means are given wrongly; it seems that he divided the standard deviation by the square root of 12, i.e. the number of items, whereas it should have been the square root of the numbers of boys and girls respectively. The standard error of the mean should therefore read 0·088 for the male 'scores' and 0·080 for the female ones.

It is only fair to add that it was possible to spot these errors only because McElroy made his numerical results available in full, a practice that deserves wider currency.

V. Subsidiary experiment and content analysis

Comparison of the Scottish and Accra preferences of both sexes combined shows that 'female' choices are nearly twice as frequent in the latter (37·2% as against 64·3%). In trying to find the factors responsible for this difference the identifications of the drawings were examined, i.e. the responses to the question 'of what does this remind you?' A strong association between preferences and naming was noticed; in other words, non-preferred pictures were rarely identified. This seemed congruent with the findings of Carmichael, Hogan & Walter (1932) on the influence of naming upon recall, and the 'effort after meaning' often encountered in experiments on perception (cf. Vernon, 1952). The causal connexion thereby suggested is that certain drawings are preferred *because* they are more meaningful to the subject than the other member of the pair. Thus it would seem to follow that the subjects found it more difficult to name rectilinear drawings than round ones, presumably owing to the fact that round objects are more common in their cultural setting than square ones.

In order to check this conclusion all the pupils in one boys' and one girls' school were given the test, but without any mention of preferences; they were merely asked to identify as many of the drawings as they could. It was predicted that a larger proportion of the 'male' pictures would be left unidentified. This proved to be the case, as is shown below:

		Percentage of shapes unidentified	
	No.	'Male'	'Female'
Boys	145	41·6	24·2
Girls	133	41·0	22·7

At the same time it is clear that the sex difference cannot be accounted for in the same way, as the proportions are closely similar for both boys and girls.

The next step was the analysis of the verbal labels attached to the shapes in the course of the main experiment. This was done in an attempt to investigate McElroy's suggestion that different associations might be called up in boys and girls viewing the same shapes.

A broad classification of responses is given in Table 2 for items nos. 2, 3 and 6; reasons for picking these three are as follows: no. 2 reached the highest level of significance; with no. 6*A* and *B* 'crown' was the dominant response of both sexes; no. 3 contains the wavy line, which will be referred to later. The remaining two significant items present no new features.

Examination of the material in Table 2 indicates the small likelihood that the sex difference could be accounted for by this factor, which is perhaps better described as that of 'familiarity'; the term 'associations' seems rather too wide, as the direction of association will itself be determined at least in part by dynamic unconscious factors such as repression.

There were only two small groups of verbal labels used by one sex and not the other; these were 'brooch', in item no. 2, mentioned exclusively by girls, and 'boxing gloves', in item no. 3, restricted to boys. There is no evidence of any important sex difference in familiarity as regards the large majority of the objects cited.

This is not to deny, of course, that such a factor did operate in individual cases. An

example is the boy who chose the pointed 'crown' and wrote that it resembles the one on the 'Cow and Gate' tin, which is quite correct.*

McElroy also thought that girls' greater intolerance of inaccuracies, for which further proof would be required, might have influenced the results. A rather superficial effort was made to find out about this by asking the boys and girls in several classes to explain their

Table 2. *Percentage distribution of different types of objects suggested by the drawings*

	Boys		Girls	
	11–13 years	14 and over	11–13 years	14 and over
Item no. 2*A*: 'female'				
Shield	38	32	10	9
Badge, emblem, crest	4	5	2	7
Other	3	3	3	4
Item No. 2*B*: 'male'				
Flower, tree, leaf	16	21	55	38
Banana, maize	20	4	9	16
Boy Scout badge	13	27	16	12
Badge or crest	5	6	4	3
Brooch	—	—	1	8
Other	1	1	1	2
	100	99	101	99
Item no. 3*A*: 'female'				
Snake	49	69	46	63
Worm, caterpillar	2	2	4	3
Rope, chain, thread	17	3	1	1
Pattern, design, 'art'	6	3	—	5
Other	4	5	2	3
Item no. 3*B*: 'male'				
Drawing, pattern, 'art'	6	4	29	7
Zigzag	7	6	5	6
Letters 'V' or 'W'	4	3	7	8
Other	5	5	6	3
	100	100	100	99
Item no. 6*A*: 'female'				
Crown	72	61	52	64
Hat, cap	7	9	4	1
Boxing gloves	1	4	—	—
Other	—	1	3	2
Item no. 6*B*: 'male'				
Crown	19	17	34	32
Hand, gloves	1	3	3	—
Other	—	5	4	2
	100	100	100	101

preferences after they had finished. As was to be expected they found this a rather difficult task. The usual response consisted of a mere reiteration: 'I like this because it looks like...'. For the present purpose it is only necessary to note that not a single subject attributed his or her preference to the fact that one shape was better drawn than the other. What was said in the introduction about the relative lack of interest in pictorial representation points in the same direction.

VI. Interpretation of the sex differences

Of the several major hypotheses advanced by McElroy only the psychoanalytic one appears to survive the process of elimination pursued in the present investigation. But

* It appears that the boy had recently been asked to purchase a tin. There is a widespread belief that Europeans always feed their infants on artificial milk, and a tendency among literates to follow what is mistakenly regarded as the better practice.

there is yet one more problem to consider, namely the decline in the older girls' preferences for 'male' shapes. The writer unfortunately cannot pretend that he was able to anticipate this—on the contrary, it came as a considerable surprise. However, brief reflexion showed that it *could* have been predicted on the basis of the psychoanalytic hypothesis.

From the writers' general studies of adolescents in Accra it would seem that a considerable proportion of young people have sexual experiences during their 'teens, and there are indications that this occurs with girls earlier than with boys. When the present problem arose the writer discussed this matter with several experienced welfare officers, probation officers and doctors in order to obtain an independent check on his impressions; their judgements coincided almost unanimously with the above conclusions.

The general trend may thus be interpreted as follows: for the boys, taken as a group, the heightening of repressed, and probably also suppressed, affect with increasing age is counteracted by partial* outlets which roughly balance the former; full sexual activity mostly occurs at a later age than that of the majority of the boys represented in the sample. In the case of the girls full outlets are achieved after about the age of fifteen, involving a lessening of repressed or suppressed affect, which is withdrawn from the symbolic and transferred to the actual objects.

It has to be admitted that this line of theorizing is *ex post* and founded on indirect inference, in the sense that the decline of repression or suppression in girls and its maintenance in boys has been assumed rather than demonstrated; but although it is speculative to some extent, the conclusion appears to fit the facts better than any alternative hypothesis.

One further statement can be made with confidence: whilst the results are consistent with the general psychoanalytic theory of symbolism, they contradict in at least one particular Freud's assertions about the meaning of symbols. Among those preferring it, there was a significantly larger number of boys than of girls who called the wavy line in item no. 3 a 'snake'; but Freud (1922, p. 167) maintained that the snake is the classical male symbol. Thus it is possible that McElroy's broader formulation concerning curved versus rectilinear forms may have more general validity, and that some of Freud's other specific interpretations which contradict it might turn out to be either false or at least misleading.

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* The term 'partial', used with reference to the group, leaves it indeterminate whether a majority of the boys obtain some limited sexual satisfactions, or whether a minority attain complete ones. There is insufficient evidence to decide this question.

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A NOTE ON THE SCALE PRODUCT AND RELATED METHODS OF SCORING ATTITUDE SCALES

By H. J. BUTCHER

Department of Education, University of Manchester

Attitude scales constructed by Thurstone's method of equal-appearing intervals may also be scored by Thurstone's method or by Likert's or by a combination of the two. Some possible advantages of a combined method are described.

Three variants of such a combined method are distinguished, and their characteristics discussed. In the case of two of these, no evidence is found to show that they possess any advantage over Likert's method of scoring, and some evidence seems to indicate that they do not. For the third (the Scale Product method of Eysenck & Crown, 1949), higher reliability is claimed by its authors, but insufficient information is available about the details of the method for this claim to be tested.

I. Thurstone's and Likert's methods of scoring

It has long been recognized that both the Thurstone and Likert methods of scoring attitude scales suffer from certain weaknesses, and several attempts have been made to develop a new method which would combine the advantages and avoid the defects of both. This note will not be concerned with alternative methods of *constructing* attitude scales; a good case appears to have been made for the superiority of the Thurstone technique of equal-appearing intervals (Ferguson, 1939), and all the scales to be discussed were constructed by that technique.

The Thurstone method of *scoring* seems more open to question. The respondent is asked to mark those items on a list of attitude statements with which he agrees, and the median scale value of these items then becomes his score. This dichotomization of the items into those 'agreed with' and 'disagreed with' is somewhat crude and fails to extract the maximum information. Not only is the respondent likely to agree to quite different degrees with the various selected items on which his score is directly based; in the case also of the items which he does not mark he would presumably express *degrees* of disagreement if the form of questionnaire permitted.

One theoretical weakness of the Likert system of scoring is equally obvious. Of two items one may have a Thurstone scale value of 9, and another, much less extreme, a scale value of 6; yet under the Likert system approval (or disapproval) of these two items will earn the same mark.

The respective advantages and disadvantages of the two methods seem, therefore, to be as follows. The Thurstone method accurately weights each item, although it does not allow for shades of agreement or disagreement; the Likert method permits a number of possible responses to each item, ranging, for example, from 'strongly agree' to 'strongly disagree', but gives equal weight to items which should ideally receive differential weighting. On balance, Likert's technique appears slightly preferable, both because scores are easier to compute and because it has been shown by Likert, Roslow & Murphy (1934) to produce rather higher coefficients of reliability. It is obviously tempting, however, to try to produce a type of scoring which will take into account both the scale values of items and the graded responses of individuals.

Three such attempts are those of Likert, Roslow & Murphy (1934), Eysenck & Crown (1949) and Castle (1953). All these newer techniques combine features of Thurstone's and Likert's methods, but they differ slightly in details of procedure and in the results claimed for them.

II. Thurstone-weighted scoring

(*i*) *General characteristics*

Likert, Roslow & Murphy describe what they call 'Thurstone-weighted' scoring as follows:

> Another method which is in a way a combination of the Thurstone and simpler (i.e. Likert) methods was also tried.... The same statements were used in this scoring as were used in the simpler method, but instead of assigning 1–2–3–4–5 to the respective alternatives, the values assigned were based upon the scale values of the Thurstone method. The 'strongly agree' alternative of each question was given the value shown by the Thurstone key for that statement.
>
> The 'strongly disagree' alternative of each statement was given a value equal to 11·0 minus the value of the 'strongly agree' alternative for that particular statement. The question mark was always given the value of 5·5 and the 'agree' alternative for each statement was given a value midway between 5·5 and the value of the 'strongly agree' alternative for the same statement. The 'disagree' position was similarly scored.

Certain features of the Thurstone-weighted technique, as described by Likert, Roslow & Murphy, merit further attention. First, the scoring of the 'strongly agree' and 'strongly disagree' responses to each item is weighted according to the Thurstone scale values of the items, and the same principle applies to a lesser degree to the 'agree' and 'disagree' responses, but the 'uncertain' responses all receive the same score, regardless of the scale value of the item concerned. This is probably a slight theoretical weakness, since a subject who is uncertain about a statement at the positive end of a Thurstone-constructed scale may well be manifesting a very different attitude from another subject who is uncertain about a statement at the negative end, yet with the Thurstone-weighted technique as with the Likert, uncertainty on either of the two items will contribute the same mark to the subject's total score.

Another characteristic of this kind of scoring is worth noting for purposes of comparison. The method of computation ensures automatically that, as with Likert scoring, the *sum* of the five weights is the same for each item.

(ii) *Reliability*

Likert, Roslow & Murphy scored several Thurstone-constructed attitude-scales both by the ordinary Likert and by the Thurstone-weighted techniques, and found high agreement between the two sets of scores, with correlations between +0·95 and +0·98. (The correlations of Thurstone-weighted scores with ordinary Thurstone scores were markedly lower, ranging from +0·83 to +0·87.) The respective reliability coefficients resulting from the Likert and the Thurstone-weighted scoring techniques were not specifically compared by the authors, who were satisfied by the high correlations between the two that little or nothing was to be gained by using the considerably more laborious Thurstone-weighted method. It is possible to extract and compare some of the reliabilities from data provided in separate tables in the article. This comparison (and tests of significance) confirm the conclusions of Likert, Roslow & Murphy. None of the differences in reliability, as between the two methods of scoring, is statistically significant (see Table 1).

III. Scale Product scoring

(*i*) *Eysenck & Crown's description*

The above comparison of reliabilities is of particular interest in view of the claim made for the rather similar Scale Product technique devised by Eysenck & Crown (1949), who describe their method as follows:

In this method, the weight given to the various responses to each item by the Likert method is multiplied by the scale position of the item as found in the Thurstone method. The rationale of this procedure is obvious. As an example, let us take two items, both of which are answered 'strongly agree', and would therefore score 5 points on the Likert system. One of these items, say, is mildly anti-Semitic, the other

Table 1

N	Attitude scale no.	Likert scoring. Split-half reliability	Thurstone-weighted scoring. Split-half reliability
61	23	+0·91	+0·86
65	30 (groups D and E)	+0·89	+0·88
50	30 (group F)	+0·89	+0·90

Table 2. Original Weights

Item no.	Scale position	Strongly agree	Agree	Uncertain	Disagree	Strongly disagree
1	3·7	5	5	4	3	3
2	7·8	1	3	4	5	7
3	4·9	5	4	4	4	3
4	8·5	1	2	4	6	7
5	1·3	7	6	4	2	1
6	2·0	7	5	4	3	1
7	4·6	5	4	4	4	3
8	9·2	0	2	4	6	8
9	2·9	6	5	4	3	2
10	6·2	3	3	4	5	5
11	0·5	8	6	4	2	0
12	6·9	2	3	4	5	6
13	7·3	2	3	4	5	6
14	1·0	8	6	4	2	0
15	8·1	1	3	4	5	7
16	1·7	7	6	4	2	1
17	6·5	3	3	4	5	5
18	8·9	1	2	4	6	7
19	2·5	7	5	4	3	1
20	5·3	5	4	4	4	5
21	5·7	3	4	4	4	5
22	3·4	6	5	4	3	2
23	9·7	0	2	4	6	8
24	4·1	5	5	4	3	3

extremely so. (The relative Thurstone scale positions might be 6 and 10, where 5 denotes neutrality.) Clearly the endorsement of the second item is a much stronger indication of anti-Semitism than the endorsement of the first, and should consequently be weighted much more heavily. This weighting is accomplished automatically by multiplying the Likert weight(s) by the scale position (6 and 10 respectively), so that the respective weights would be, according to the method of Scale Products, 30 and 50.

This method gives rather unwieldy numbers, and can be improved by dividing all products by a constant, and rounding off. This brings the scale products down to manageable proportions. The actual values finally selected for the anti-Semitism scale are given in table 2, under the heading 'Original Weights'.

Eysenck & Crown's table 2 is reproduced as Table 2 above.

(*ii*) *Reliability*

Eysenck & Crown then report that this Scale Product method of scoring, when applied to their scale of anti-Semitism, produced a reliability of +0·94, which was significantly higher at the $P = 0{\cdot}01$ level than either of the corresponding values produced by the Thurstone and Likert methods. Eysenck (1954) also claims that: 'This new method has been shown to be more reliable than the other two, and it might be made more reliable still by using an empirical weighting system rather than the fairly mechanical one shown in the text.' This claim is important but appears to rest on the scoring of only one scale. The present writer had hoped to test it further, but encountered certain difficulties in interpreting Eysenck & Crown's description of the technique.

(*iii*) *Some difficulties of interpretation*

First, it obviously will not do simply to multiply the Thurstone scale value of each item by the Likert weights 1–2–3–4–5, which is what Eysenck & Crown might seem to suggest. Suppose one has two items whose scale values are 3 and 10. The 'strongly agree' response of the first item would then be scored 15, and the 'strongly disagree' response 3. Similarly, the second item (with scale value 10) would receive a score of 10 for the 'strongly agree' response and a score of 50 for the 'strongly disagree' response. But in this case the latter item (and all the other items with high scale values) would exercise a disproportionate influence on each respondent's total score, although the direction of scoring is of course arbitrary for an attitude scale, and the scale values might equally well run from 10 to 0 as from 0 to 10. Presumably the correct procedure is to express each scale value as a deviation from the theoretical neutral point of the scale before multiplying it by the Likert weights. This, as we shall see later, is what Castle does.

Furthermore, on turning to the table of weights, it is very difficult to see how Eysenck & Crown's figures can be obtained by even a modified version of the procedure they recommend. In Table 2 all the 'uncertain' replies receive the same score, regardless of the scale value of the item. If, however, the different scale values of the items (or the different deviations from neutrality) are multiplied by Likert weights and are then all divided by a constant, which is what Eysenck & Crown appear to suggest, the 'uncertain' responses ought surely to receive differing Scale Product weights in the same way as all the other responses. Secondly, for each item the five weights add up to 20 (except in one case, which is clearly a misprint). If the scale values or deviations had been multiplied by Likert weights and then divided by a constant, it is hard to see how this could have occurred.

At first sight, an alternative interpretation of Eysenck & Crown's description of the procedure might be that *for each item* the Scale Product weights are divided by a constant, or in other words that one constant is not used for the whole table but a number of different constants, such that for every item the 'uncertain' response is scaled down to a value of 4. But it turns out that this procedure could not produce the set of figures in the table. All it would produce would be a set of slightly magnified Likert weights (1–3–4–5–7) identical for each item.

The more one examines the table in Eysenck & Crown's article, the more difficult it becomes to reconcile it with the account in the text, for the table seems to have been constructed by some method rather more analogous to that used by Likert, Roslow & Murphy (1934). At least it has been shown to possess two features which characterize their

method (all 'uncertain' responses scored alike and equal sums of weights for each item), but which are hard to reconcile with Eysenck & Crown's own description.

This analysis of Eysenck & Crown's method is necessarily detailed and critical. It should be added that their account of the Scale Product method is highly condensed and forms only a small part of a long and important paper.

IV. Castle's Scale Product method

(*i*) *General characteristics*

The third paper is that of Castle (1953), who uses the method *advocated* by Eysenck & Crown with one or two unimportant variations. Although he makes no claim to originality of technique, his is the only work known to the writer which describes the actual employment of this method. It will therefore be convenient to refer to it as 'Castle's method' to distinguish it from the method exemplified in Eysenck & Crown's table.

Castle's scale products are obtained by first computing the deviation of each item's scale value from the neutral point of the scale, and then multiplying this deviation by the Likert weights; the 'uncertain' responses thus receive different Scale Product weights according to item in the same way as the other responses, and the sum of five weights assigned also varies according to the scale value of the item. One slight peculiarity of his method is that he takes 6·5 as the neutral point of the scale, although his items were originally sorted into 11 piles.

A more serious point of criticism arises from his use of minus weights. The three methods previously discussed which allow for graded responses (i.e. the Likert, Thurstone-weighted, and Eysenck & Crown methods) all use reversed scoring for half of the items in the scale. With the Likert method, for example, it is usual for the items falling at one end of the continuum to be scored 1–2–3–4–5 and those at the other end 5–4–3–2–1. The same principle applies to the Thurstone-weighted and Eysenck & Crown methods, which possess the property that if the scale values of two items are equidistant from and on different sides of the zero point, these items will receive the same weights but in reverse. Thus in all these methods the *range* of weights for the different items is the same for the 'strongly agree' responses as for the 'strongly disagree'. As a consequence of his use of minus weights, this is not true of Castle's scoring, and the defect is aggravated by the large numbers he uses. The result is that the weights he assigns to 'strongly agree' responses range from +23 to −21 (for the 5 items tabulated in his paper), and those of the 'strongly disagree' only from +5 to −4. This sort of asymmetry seems likely to produce some curious results.

(*ii*) *Reliability*

Castle, following Eysenck & Crown, suggested that his scale was probably more reliable than if it had been scored by the Likert method. But, as we have seen, Castle's method differs in principle from that used by Eysenck & Crown, as inferred from their table of weights. It therefore seemed of interest to determine empirically whether a type of Scale Product scoring approximating to Castle's would produce a higher split-half coefficient of reliability than the ordinary Likert method.

The material used consisted of Part I of a questionnaire on attitudes to education constructed by Oliver (1953). This scale contained 30 items, of which the following are examples: 'Mathematics is valuable for the training it gives in abstract reasoning', and

'Naturalness is more important than good manners in children'. On a sample of 57 Manchester University evening students it had been found to have a split-half reliability (corrected) of +0·92 when scored by the Likert method.

Scale Product weights were calculated on the same principle as Castle's, and the same 57 answer sheets re-scored. The scoring technique differed from Castle's in the following respects: (*a*) the theoretical mid-point of the scale was taken to be not 6·5 but the actual mean scale value of the 30 items, (*b*) reversed scoring was used for half of the items in order to preserve the symmetry of the scale, (*c*) the resulting Scale Product weights were scaled down, as recommended by Eysenck & Crown, both in order to avoid unwieldy numbers and to make them more comparable with Likert weights. The split-half reliability (corrected) was now found to be +0·89. This is lower than the previous value, but a test of significance (Fisher's *z* test) showed the difference not to be significant at the $P = 0{\cdot}05$ level.

V. Comparing reliabilities: some further considerations

To apply a test of significance of this kind to the difference between two reliability coefficients is possibly of doubtful legitimacy when these have been obtained from the same data. The assumption underlying such a test is that the data are independent and uncorrelated, so that both the above comparison and the earlier one between the reliability of the Likert and the Thurstone-weighted methods should be treated with caution. The same is true of Eysenck & Crown's claim for the superior reliability of their Scale Product method. On the other hand, it may well be that the result of applying such a test of significance to coefficients obtained from the same data is to *underestimate* the significance of the difference. In this case it would be only the negative results of the tests of significance reported in the present paper that should be considered suspect. The main point of both comparisons, that neither the Thurstone-weighted nor the Castle technique is more reliable than the Likert, remains in any case unaffected.

Another consideration relevant to the above comparisons of reliability has been mentioned by Vernon (1953), who suggests that the use of both Thurstone-type scaling and item analysis in scale construction tends towards higher reliability. Item analysis does not appear to have been used in the construction either of Eysenck and Crown's or of Castle's scales, but was used in addition to Thurstone scaling in the construction of Oliver's. Thus it is possible that the Scale Product method, in the version either of Eysenck & Crown or Castle, is more reliable than Likert scoring when applied to an ordinary Thurstone scale, but not when applied to one that has been modified by item analysis as used in the Likert method of construction.

I should like to thank Prof. R. A. C. Oliver and Prof. P. E. Vernon for their generous encouragement and helpful criticism during the writing of this note.

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AN INVESTIGATION INTO THE PERCEPTION OF HEIGHT BY RATS

By NIROD MUKERJI

Gauhati University, Assam, India

Three rats were made to jump a barrier to reach food. If the barrier was of uniform height, rats showed no preference for negotiating any particular part of the barrier. If any part of the barrier was lowered by even a small amount, the rats showed preference for this portion. If the barrier was uniformly raised to a height which was not jumped, and then part of it was raised yet higher, the part of the barrier remaining relatively low was jumped successfully. It is concluded that if part of the barrier appears relatively low, the rat may 'decide that it can jump it after all'.

I. Introduction

The experiments described were the outcome of observations made on stray cattle attempting to jump into a plot surrounded by a fence, with a gate of the same height as the fence. They were unsuccessful until the height of the fence was raised 1 ft., the gate being left at the original height. The animals now jumped the gate, which seemed low compared with the rest of the fence. Was this behaviour due to chance factors, or was the animals' success in jumping governed by comparative rather than absolute heights? The following experiments, using rats, suggest that comparative height is an important factor determining an animal's 'estimate' of what it can jump.

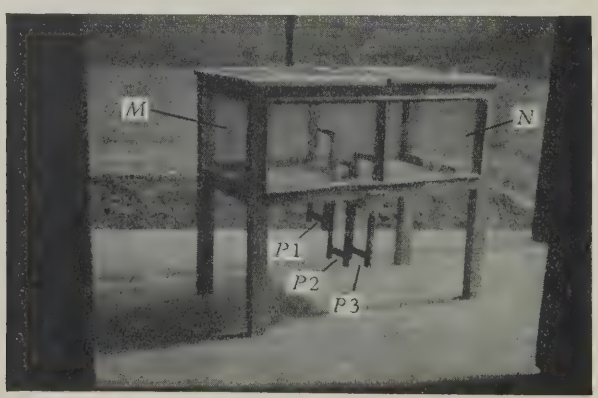

Fig. 1. The cage is closed with the partitions $P1$, $P2$ and $P3$ standing at different heights; M and N are the two halves of the cage.

II. Apparatus

A cage of wood and wire meshing, 4 ft. long, 2 ft. wide and 2 ft. tall, was divided in two by a fence consisting of three equal partitions, made of glass in wooden frames. The partitions could be raised and lowered individually.

III. THE EXPERIMENTS

Healthy common rats of similar size and growth were used. Before experiments they were starved for 24 hr. in a separate cage. The rats were tested individually by placing them in one-half of the experimental cage, food being placed at the mid-point of the remote end of the other half. If the rat jumped the fence, the food was immediately removed and placed in the first half of the cage; and so on. The rats were rested if they showed signs of fatigue or lack of interest. If the rest pause was ineffective, the experiment was suspended for the day, and the rat fed. Five rats started the experiment, but one died and another escaped before the finish.

With all the partitions at the same height, rats tended to jump over the partition nearest the room wall. When the cage was placed so that it was uniformly lit, the rats jumped over any one of the three partitions, apparently at random. As a further control, the cage was moved through 45° between each jump. There were three parts to the experiment. A count was kept throughout of which partitions the rats negotiated. The sequence of presentations was as follows:

A. (*a*) Fifteen jumps, for each rat, with all three partitions (Fig. 1, $P1$, $P2$, $P3$) at the same height of 1·5 in.
   (*b*) Five jumps with $P1$ at 1 in., $P2$ and $P3$ still at 1·5 in.; then similarly with $P2$ and $P3$ lowered in turn, the other two partitions in each case remaining at 1·5 in.
   (*c*) Each partition in turn raised to 2 in., the other two being kept at 1·5 in., five jumps being made by each rat under each condition.

B. (*a*) Fifteen jumps by each rat with $P1$, $P2$, $P3$ all at 6·0 in.
   (*b*) Each partition in turn raised to 6·25 in.; five jumps in each case.
   (*c*) As for (*b*), but raising each partition in turn to 6·5 in.
   (*d*) As for (*b*), but lowering each partition in turn to 5·75 in.
   (*e*) As for (*b*), but lowering each partition in turn to 5·5 in.

C. (*a*) All three partitions were gradually raised together until the rat failed to jump them in three successive attempts. Fatigue was minimized by taking several days over this process. The final height reached varied from rat to rat. This height was taken to be just beyond the 'psychological limit' for the rat.
   (*b*) Starting with all partitions at this height, each portion in turn was lowered by 0·25 in., i.e. to a height which should have been negotiable. If the rat jumped successfully, two more jumps (though not in succession) were allowed for that partition. Similarly for the other two partitions.
   (*c*) As for (*b*), but each partition in turn being lowered by 0·5 in.
   (*d*) As for (*b*), but each partition in turn being raised by 0·25 in., i.e. the barrier was altogether above the previous 'psychological limit'.
   (*e*) As for (*d*), but each partition in turn being raised 0·5 in.

IV. RESULTS

Table 1 represents sum total of successful jumps for the three rats with all the partitions, $P1$, $P2$ and $P3$, at 1·5 in. (A (*a*)); the adjusted partitions lowered at 1 in. and the other two* at 1·5 in. (A (*b*)); and the adjusted partitions raised to 2 in. while the other two

* It was not possible to determine with certainty as to which of the two partitions standing at the same height was selected, since often when the partitions stood side by side the rats jumped over the line of contact.

remained at 1·5 in. (A (*c*)). In this and the following tables P values indicating significance at the 0·05 level or better are shown; in all other cases selection of partitions may be ascribed to chance.

Table 2 likewise represents the successful jumps under five varied conditions.

Table 3 represents successful and unsuccessful jumps at the limiting height (l.h.) of the partitions with one adjusted.

Table 1

	Partitions	Jumps	χ^2	d.f.	P
A (*a*)	*P*1	16	0·12	2	—
	*P*2	14	—	—	—
	*P*3	15	—	—	—
A (*b*)	Adjusted one	35	38·02	1	<0·001
	The other two*	10	—	—	—
A (*c*)	Adjusted one	7	5·62	1	<0·02
	The other two	38	—	—	—

* Twice as many jumps are expected by chance from the two partitions not adjusted as for the one adjusted.

Table 2

	Partitions	Jumps	χ^2	d.f.	P
B (*a*)	*P*1	16	0·13	2	—
	*P*2	15	—	—	—
	*P*3	14	—	—	—
B (*b*)	One raised to 6·25 in.	9	3·02	1	—
	Others at 6 in.	36	—	—	—
B (*c*)	One raised to 6·5 in.	3	13·25	1	0·001
	Others at 6 in.	42	—	—	—
B (*d*)	One lowered to 5·75 in.	28	16·62	1	0·001
	Others at 6 in.	17	—	—	—
B (*e*)	One lowered to 5·5 in.	35	38·02	1	0·001
	Others at 6 in.	10	—	—	—

Table 3

C (*b*)	One lowered by 0·25 in.	15	20·63	1	0·001
	Others at l.h.	2	—	—	—
	Unsuccessful jumps	10	—	—	—
C (*c*)	One lowered by 0·5 in.	21	39·04	1	0·001
	Others at l.h.	0	—	—	—
	Unsuccessful jumps	6	—	—	—
C (*d*)	One raised by 0·25 in.	1	0·03	1	—
	Others at l.h.	4	—	—	—
	Unsuccessful jumps	22	—	—	—
C (*e*)	One raised by 0·5 in.	0	2·16	1	—
	Others at l.h.	7	—	—	—
	Unsuccessful jumps	20	—	—	—

V. Discussion

It can be concluded that rats prefer to jump the lowest part of a barrier, even though the difference in height is as small as between 5·75 and 6·0 in.

Although there is a tendency for the lower partitions to be chosen more often the greater the absolute difference in height of the partitions, this result is nowhere significant.

The results of experiment C show that a height, previously not jumped, may be jumped if it appears low in comparison with some other part of the barrier. This suggests that relative lowness is an important factor in determining a rat's jumping behaviour, a factor which may override a rat's 'assessment of the maximal height it can jump'.

The writer is indebted to B. M. Foss, Birkbeck College, London, for help with the manuscript.

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THE EFFECT OF STIMULUS LIGHT INTENSITY ON REACTION TIME OF SCHIZOPHRENICS

By P. H. VENABLES and J. TIZARD

Medical Research Council, Unit for Research in Occupational Adaptation, Maudsley Hospital, London

It is a commonly accepted dictum that schizophrenics show signs of withdrawal under threat of 'punishment'. A possible explanation is that any stimulus producing a larger than optimum degree of excitation will bring about a worsening of response in these patients.

An experimental test of this suggestion was attempted by means of a reaction time (R.T.) study with four different intensities of stimulus light. It was postulated that the higher values of stimulus intensity would produce an effect of increase in reaction time in schizophrenic patients, in contrast to the well-established finding with normal subjects of a non-linear decrease in R.T.

A standard reaction-time procedure was used. The subject was asked to release a Morse key as quickly as possible after the onset of a stimulus light which was presented once every 8 sec. A forewarning buzzer preceded the light at a 3 sec. interval. The timing of buzzer and light was carried out electronically.

Four intensities of stimulus light were used. These were: *A*, 0·26 W., *B*, 0·52 W., *C*, 1·33 W., *D*, 2·25 W. The surfaces surrounding the stimulus light and response key were painted matt black to avoid reflexions.

Twenty presentations of each light intensity were made, order effects being controlled by the use of balanced Latin square design. Three minutes' rest was allowed between each set of twenty presentations.

Six groups of four non-paranoid chronic schizophrenic subjects were used, each group completing a Latin square design.

Two groups of four nurses were used as normal controls. Four of the schizophrenic groups were made up of male and the remaining two of female patients.

The mean age of the male patients was $36{\cdot}0 \pm 5{\cdot}9$ years, of the females $39{\cdot}3 \pm 11{\cdot}2$ years, and of the normal controls $30{\cdot}1 \pm 2{\cdot}1$ years.

In working out the results only responses 6–15 were scored to avoid possible warming up effects and end spurts. The analysis of all twenty responses would, however, have given results similar to those presented here.

The main finding is shown graphically in Fig. 1. In each of the six patient groups after a decrease in reaction time from intensity *A* to intensity *B*, there is an increase in reaction time with intensity *C* and a subsequent decrease with *D*. In the normal group, however, there is a non-significant decrease ($P > 0{\cdot}05$) in reaction time from *A* to *B*, no increase in reaction time from *B* to *C* and an insignificant increase ($P > 0{\cdot}1$) with *D*.

In the schizophrenic subjects the decrease in reaction time from *A* to *B* is significant at the 0·1% level ($t = 4{\cdot}45$). The increase in reaction time from *B* to *C* is significant at the 1% level of confidence ($t = 3{\cdot}26$); the subsequent fall from *C* to *D* is not significant

($t = 1{\cdot}63$). No effect of order of presentation was found ($F = 0{\cdot}012$). The 3 min. rest period between presentations can thus be considered adequate.

The decrease in R.T. between stimulus intensity *A* and stimulus intensity *B* found in twenty-three out of twenty-four of the schizophrenic subjects is in line with that found in normal subjects, but is approximately three times as great as that found with the normal controls tested in this experiment. The increase in reaction time with higher stimulus intensities found in twenty-one out of the twenty-four schizophrenic subjects tested is a

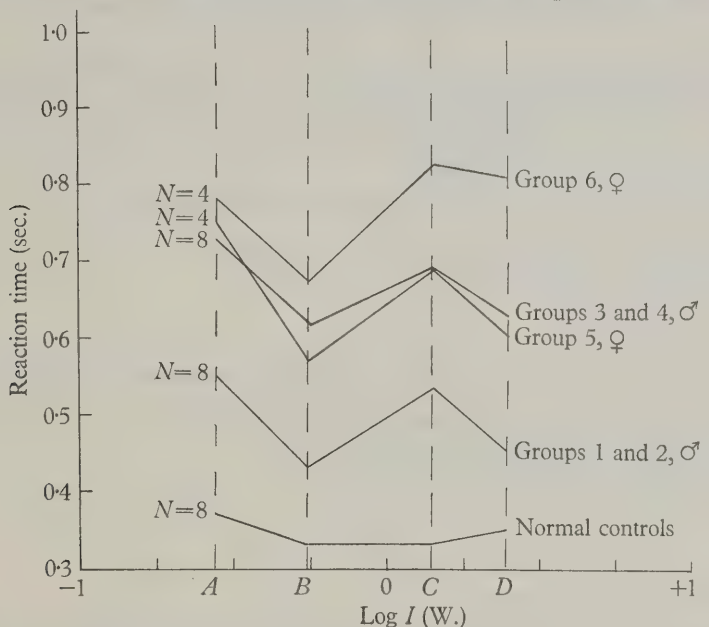

Fig. 1. Reaction times of schizophrenic and normal groups plotted against logarithm of intensity of visual stimulus.

finding which is surprising in its regularity and is in some ways akin to the paradoxical effect found in conditioned reflex studies. The decrease in reaction time in schizophrenics from stimulus *C* to stimulus *D*, although non-significant, is worthy of further experimentation. Possible explanations of this are: (*a*) the non-continuance of the earlier effect; (*b*) the subject looks away from the brightest stimulus and receives effectively less excitation on the fovea; (*c*) the retina is in a refractory phase after strong stimuli due to the presence of after images.

Further experimental work is necessary to test these explanations, and it is hoped to carry out a further experiment with greater control of the stimulus conditions and a larger range of light intensities.

Schizophrenic groups (3, 4 and 5) were at the time of the experiment receiving treatment by chlorpromazine (Largactil); the remainder were receiving neither treatment nor sedatives. It is evident that the presence of the drug does not change the essential pattern of results.

This study was carried out at Springfield Hospital, London, by the kind permission of the Medical Superintendent, Dr H. C. Beccle.

(*Manuscript received* 13 *July* 1955)

THE PERFORMANCE OF ONE-YEAR STUDENTS IN THE DEPARTMENT OF EDUCATION, MANCHESTER UNIVERSITY

By F. W. WARBURTON

Department of Education, University of Manchester

This report is concerned with the performance of 95 one-year students in the Department of Education, University of Manchester, in the session 1952–3 and 105 students in the session 1953–4. The students are classified according to sex, and faculty (Arts or Science). Eight predictors of success are considered.

In the School Certificate Examination (all examination boards):

(i) Number of passes, credits and distinctions (total subjects).
(ii) Number of credits and distinctions.
(iii) Number of distinctions.

In the Higher School Certificate Examination (all examination boards):

(iv) Number of scholarship and principal subjects (total subjects).
(v) Number of scholarship and good (principal) subjects.
(vi) Class of University degree (all universities).
(vii) Interview grade.
(viii) Grade of referee's report.

There are two criteria of success:

(i) Theory mark (examination papers plus essays) in the Graduate Certificate of Education.
(ii) Practical teaching mark in the Graduate Certificate of Education.

Product moment correlations were calculated between the predictors and the two criteria of success for the two years combined. (They were very consistent from year to year.)

	Criteria	
Predictors	Theory mark	Practical teaching mark
S.C. total subjects	+0·187**	+0·043
S.C. credits and distinctions	+0·220**	−0·061
S.C. distinctions	+0·168*	−0·019
H.S.C. total subjects	+0·161	−0·011
H.S.C. good (principal) and scholarship subjects	+0·117**	−0·046
University degree (Hons. *v.* Ordinary)	+0·433**	+0·197*
Interview grade	+0·383**	+0·348**
Referee's report	+0·320**	+0·299**

(Significance of correlations: ** highly significant (0·01); * significant (0·05).)

The correlations between the predictors and the criteria of success are rather low, ranging from −0·011 to +0·433. A candidate's academic record at school significantly

predicts success only on the theoretical side of the course. The grades given by the referees and by the interviewers are both more predictive than examination results, and are correlated with success in both the theory papers and in practical teaching.

Few significant differences in the variables studied were found between the mean scores of groups of students. Arts students, compared with Science students, have gained more credits and distinctions in the School Certificate Examination, receive higher grades in referees' reports, and score higher in the theoretical papers in the Graduate Certificate of Education. There are no significant differences between the sexes.

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A NOTE ON THE BASAL LEVEL OF SKIN CONDUCTANCE AND BINET I.Q.

By N. O'CONNOR and P. H. VENABLES

Medical Research Council, Unit for Research in Occupational Adaptation, Institute of Psychiatry, London

A group of imbecile women tested on a resting GSR level, namely micro-mhos conductance and on a dexterity test (GATB tests O and P) as well as on the Binet Scale (Stanford, 1937 Revision) showed a correlation of $-0{\cdot}4$ ($P = 0{\cdot}02$) between Binet I.Q. and resting conductance level. It was inferred that male imbeciles would have a significantly higher level of conductance than an equivalent normal male sample. This was found to be the case ($t = 2{\cdot}75$, $P < 0{\cdot}01$). The results suggest a relationship between mental performance and autonomic reactivity which deserves further investigation.

Introduction

In connexion with work on learning, motivation and performance (O'Connor & Claridge, 1955), imbecile women were tested on three measures: the Binet (Stanford, 1937 Revision), the finger dexterity tests of the United States Employment Service Battery (called O, assembly, and P, dis-assembly) and a valve voltmeter type of circuit measuring skin conductance. The purpose of these tests was to apply them as controls in eliminating the effects of initial intelligence, dexterity and emotional, or, more correctly, autonomic labilty on subsequent learning or performance.

Method and results

Forty imbecile women between the ages of 15 and 37 were selected for the larger experiment. On the basis of I.Q.'s and other considerations, such as the existence of epilepsy or the fact that some were receiving drugs, 5 were eliminated. The remaining 35 were of Binet mean I.Q. 36, mean dexterity (O plus P) 24, and mean resting conductance 9·5 micro-mhos. Standard deviations were 4·4, 6·7 and 5·5 respectively. The mean age of the group was 28 years. The scores were intercorrelated with the following results:

	r	*P*
Binet I.Q. with conductance	−0·40	0·02
Binet I.Q. with finger dexterity	0·48	0·02
Conductance with finger dexterity	−0·21	0·10

Skin conductance was measured under conditions wherein effective temperature did not exceed 66° F. Venables (1955) has suggested that higher temperatures affect basic conductance. Each subject's resting level was assessed as a level reached and sustained after a period of accommodation lasting 20 minutes. These results were accepted as indicative of a possible relationship between resting conductance level and I.Q. in imbecile women. It was inferred that the conductance of imbecile men should be significantly greater than that for normal men. A random selection of 18 male imbeciles of equivalent age and I.Q. was compared with 18 normal men matched for age (Venables, 1955). The means and standard deviations in these samples were:

	Mean	S.D.
Imbecile men	15·36	6·83
Normal men	9·6	2·8

A t test made between the two male groups, one normal and one imbecile, yielded a t of 2·75, equivalent to a P value of 0·01. In this case resting conductance was measured after 4 minutes for both groups.

Discussion

Such a relationship as the one reported has not been found in the literature. A finding which may bear some similarity to this is found in Sleeper & Jellinek's (1936) work on polyuric and non-polyuric schizophrenics, in so far as they believed that their two groups differed in sympathetic reactivity and at the same time in I.Q. There are many possible physiological and biochemical explanations of this cortico-visceral interaction which might be offered, but which the authors are not qualified to evaluate.

The authors wish to acknowledge the considerable help afforded by Dr J. F. MacMahon, Physician Superintendent of the Manor Hospital, Epsom, England, who kindly gave every facility for the work, and that of his staff who made the subjects available.

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(*Manuscript received* 5 *September* 1955)

PUBLICATIONS RECENTLY RECEIVED

Essays on the Patho-Physiology of the Higher Nervous Activity. By A. G. Ivanov-Smolensky. Moscow: Foreign Languages Publishing House. 1954. Pp. 348. 7*s.* 6*d.*

On but few occasions during recent years has it been possible for outsiders to have other than a very fleeting glimpse of developments in medicine, physiology and psychology within the Soviet Union. Reports from those few who have visited Russian laboratories, and papers read by Soviet scientists at international congresses, have suggested that Pavlovian theory and method are playing central roles in these developments. Therefore, it is of great interest that an English translation of this series of essays has been published. The author was one of Pavlov's colleagues, and the essays were written as a tribute on the occasion of the centenary of Pavlov's birth. The book was awarded a Stalin Prize in 1949, an indication that it accurately presented Russian views in the biological fields with which it is concerned.

There are three main essays, each concerned with a different aspect of the evidence upon which Pavlov's theory of 'the higher nervous activity' is based. The main features of the theory are familiar and are based upon observations employing 'the method of conditional reflexes'. The first two essays discuss research on the 'pathological changes' in conditioned behaviour associated with experimental lesions of the cerebral hemispheres, with exposure to 'difficult tasks' ('experimental neurosis'), with artificially induced disturbances of the internal milieu of the organism (e.g. the endocrine system), with experimental intoxication (e.g. 'morphine intoxication'), and with certain infections (e.g. 'pasteurellosis'). During the course of this research '. . . it proved possible to create in the laboratory not only experimental (though roughly simplified) models of neurotic syndromes, but also models of different (in particular, toxicogenic) psychotic states ('experimental psychoses'). Experimental therapy of these pathological states also receives considerable attention, with emphasis on retraining, on medication (e.g. bromides, caffeine, phenamine), and on sleep (natural and induced). Because Pavlov assumed '. . . that the fundamental laws of nervous activity must be identical in man and in animals, both in normal and in pathological states. . . ' it is logical that he and his colleagues should have generalized theory and experimental results from their infra-human 'models' to man. The third essay is devoted to these generalizations as they have been applied in the psychiatric and in the neurological clinic.

The essays provide a very favourable testimonial for comprehensive research planning on a grand scale, in this instance covering some fifty years and still in progress. Throughout the essays, as throughout this long period of research, Pavlovian theory serves the dual role of integrating the results of numerous individual projects and of generating new ideas for further investigation. Even those who are critical of certain aspects of the theory would agree that it has served these two roles well.

A book of this kind is likely to do more than merely satisfy a reader's curiosity regarding trends in research and theory construction. It may create a desire to do something with the information it imparts—to examine certain research studies in greater detail, to try out in the laboratory or the clinic some of the methods and ideas described, to see whether or not the research data can be fitted into other theoretical frameworks. To do any of these he will need more detailed information than any single book covering such a wide range of research could be expected to provide. In 240 pages of text are summarized reports to which references are made in 50 pages of bibliography, and this does not include all of the work done in Pavlov's laboratories. The author has described new ideas, new methods and new results, but to compress so much material into such a limited space has meant restricting the amount of detail which could be included. He has referred to research papers containing the details, but here the non-Russian reader encounters language difficulties which prevent him from exploring matters of particular interest or examining conclusions in greater depth.

Language difficulties may also account for the fact that developments in 'patho-physiology' outside Russia receive but little attention in the essays. One of the author's objectives was '. . . to expound . . . the basic achievements of this young branch of science'. To attain this objective would require an integration of the considerable amounts of research done by workers outside as well as inside Russia.

The main contribution of the essays to the 'patho-physiology of the higher nervous activity' is '. . . to give an outline of its development in the Pavlov school (including its ramifications) up to our time'. 'The Pavlov school' is taken very literally to refer to Russian scientists who have worked within the frame of conventional Pavlov theory. It does not include workers from other countries who made contributions under Pavlov's guidance, but who later altered certain aspects of his point of view. To place the essays

in an adequate perspective as regards the entire field of 'patho-physiology' it is important to keep these limits of the author's objectives in mind. This tribute to Pavlov is not concerned with his widespread influence on thinking and research beyond his own laboratories and clinics.

It is also important for the reader to remember that the School's definition of 'psychology' differs from that generally accepted in this country. '...Pavlov invariably and in every possible way avoided interpreting the resulting disturbances from a psychological point of view: he always strove to study the disturbances of the neurophysiological functions, the derangements in the dynamics of the higher nervous processes....' Such 'dynamics' are generally inferred from observations of behaviour employing 'the method of conditional reflexes'. This general approach also characterizes the theory construction of contemporary psychologists, e.g. Tolman and Hull. Psychologists generally will feel quite at home in reading of Pavlov's attempts '...to superimpose a psychopathological pattern on the neurophysiological canvas'.

The important role which 'the Pavlov school' plays in contemporary Russian psychology, physiology and medicine is reflected in the essays, and provides added incentive to examine their contents carefully. If they are made generally available they may serve as an important step in integrating Russian information in the field of 'patho-physiology' with comparable information from other countries. R. W. RUSSELL

Entwicklungspsychologie des zeichnerischen Gestaltens. By G. MÜHLE. Munich: Barth. 1955. Pp. viii+165. DM. 24.

The term 'gestalten', verb-noun of gestalt, has no English equivalent. It means a selecting, forming, structuring activity which is part of the process of producing a work of art. But it is also used in the wider sense to describe the process of shaping and structuring experiences (Erlebnisse) in any medium. Mühle takes as the subject of his book, first, the problem of gestalten as such and, secondly, gestalten in drawing as a contribution to a general developmental theory of gestalten. This book is divided into three sections, namely, the problem of pictorial ability, the process of gestaltung, and fundamentals of a theory of gestaltung in drawing.

There is very little information on the material on which the author bases his investigations or on the methods by which he arrives at his conclusions. He claims to have traced the development of free drawing in childhood up to about the tenth year, but there is no statement as to the number of subjects or the conditions under which the drawings were produced. The term 'free drawing' is not defined but, from the illustrations, it looks as if a large proportion of the drawings were executed on instruction, under class-room conditions, with a given subject (e.g. our village), or even a model such as a bowl of fruit.

The gestalt approach forms the theoretical basis of the investigation described as the 'genetic psychology of wholes and structures' of Krueger 'completed and systematically built up' by Wellek. The author claims that he is not dogmatic as to his theoretical approach, yet he makes very definite reservations by excluding literature in the fields: of comparative psychology of gestaltung, 'because fundamentals for such a task are as yet not sufficiently clarified'; of typology, 'because a true typology of drawing gestaltung promises to be fruitful at later age groups only'; and of depth-psychology, 'because the far reaching separation of conscious and unconscious, so important in adulthood, would only be found as the result of development and not in comparable form in the child'.

The method of presentation is discursive, the reader having to follow the tortuous course of critical argument with some 200 books and articles in reference to the author's statements. These references are quoted in full and, along with numerous cross-references, continually interrupt the flow of the book.

Generally speaking, Mühle finds that the development of gestaltung leads from the undifferentiated to the differentiated. A pattern of gestaltung has to be acquired and established at each stage of development, 'up to the highest planes of artistic production', before an individual 'loosening' towards expression can be attempted. This continuous patterning process is, however, superimposed by a theory of values. 'Gestaltung—even that of children—is distinguished by a definite striving towards the realization of values.'

Although the author tries to keep a fair balance in discussing the different aspects of gestaltung, he sets up a hierarchy that leads from the exclusive dominance of 'tactual-motor and emotional factors' in early childhood to the 'true' gestalten which (following Klages) 'is deeply penetrated by a noetic and volitional attitude and integrated in consciousness in a peculiar way'. Accordingly, 'true' development tends towards the representational, and it is the increasing dominance of intellectual factors which determines the degree of 'trueness' of gestaltung. The author is, however, well aware of the influence of

symbolism and motivation. He mentions, for example, a compulsive urge of children to close gaps and interprets it as a desire for security. But he brushes aside anything which would involve him in an argument with depth-psychology and, thereby, reduces both the scope and validity of his arguments.

Apart from the general theoretical weakness, there is a wealth of valuable and interesting detail. The chapter on 'Abstraction and concretion' is well-reasoned and convincing and should go a long way to clear misconceptions regarding the 'abstract' art of children.

MARTHA VIDOR

Nebraska Symposium on Motivation. 1955. By MARSHALL R. JONES (ed.). Lincoln, Nebraska: Univ. of Nebraska Press. 1955. Distributed in United Kingdom by Bailey Bros. and Swinfen, Ltd., London. Pp. x+274. 24*s*.

This is the report of the third annual Symposium on 'Current Theory and Research in Motivation' at the University of Nebraska. Each of the six symposiasts contributes a paper, which is followed by comments from other participants.

In the first paper, Maslow urges psychologists to give up their detachment and to accept an obligation to offer advice on vital human problems. The importance of these problems seems to him to justify recourse to 'inexact and unreliable researches' where others are lacking. The rest of his paper consists of a demonstration that 'inexact and unreliable researches' can lead different psychologists to very different conclusions, so that the net gain from them is not obvious. There are plenty of ungracious people about who remain to be convinced that psychology should not be done away with. They will claim that the only thing they are ever told by a psychologist, that they would not have heard from somebody else if psychology had never existed, is how wrong other psychologists are. One must confess, without denying that Maslow makes some good and telling points, that it would be embarrassing to have one of those killjoys looking over one's shoulder, just when one happens to be reading this paper of his.

There is no danger, on the other hand, in allowing the killjoy access to the appetizing stuff served up by Olds. He refurbishes some of the essential concepts, sums up recent work on the reticular formation and the limbic system, including his own epoch-making experiments which might mean the discovery of a 'reward centre' in the brain, and offers a theory which serves to tie existing data together and guide future research. In its choice of ingredients, its mode of preparation and its judicious blending of cautious assertation and piquant speculation, his contribution is exemplary.

The remaining papers lie between these two poles in manner and in matter. Young sums up some important findings on the relations between palatability of food and motivation in rats. He claims that S-R formulations could not be reconciled with these findings, but he does not make clear which S-R formulations he is thinking of and precisely what reasoning brings him to this conclusion. Peak and Rotter offer theories of human motivation which reflect some dissatisfaction with current conceptions derived in large measure from animal experiments. Their theories are eclectic in origin but seem much of the time to be rephrasing common notions in technical language—e.g. that what one does depends on what one would like and what one thinks will lead to it. Such theories may allay some impatience with the slow process of working one's way up from simple behaviour in simple organisms, since they seem to be directly concerned with everyday human activities. But their fruitfulness, no less than that of animal experiments, can only be proved in the not too near future. A promising intermediate position is represented by McClelland, who builds on theories derived from animal psychology but is not afraid to modify them radically and to use clinical-psychology techniques to test them. The researches he reports appear, however, to have more practical and methodological than fundamental theoretical importance.

D. E. BERLYNE

Psychoanalysis and Ethics. By L. S. FEUER. Oxford: Blackwell Scientific Publications. 1955. Pp. vi+134. 30*s*.

This essay is divided into three parts. In Part One the author is primarily concerned to propound the thesis that psychoanalysis gives us a method of 'verifying the ultimate value' of a society. To assert that a value is 'ultimate' is, in effect, to assert that 'there is a corresponding unconditioned and irreducible drive in the human organism'. We 'verify' this value by (I take it) applying the psychodynamic generalizations of psychoanalytic theory to discover whether a given value is 'expressive of a primal drive of the organism or whether it is anxiety-induced'. In this way ethics becomes a branch of social science, and the 'psychoanalytic criticism of values is thus the primary work of the ethical philosopher'.

Part Two examines and rejects Freud's pessimistic view of civilization as being necessarily incapable of bringing happiness to men. In the brief Part Three the author raises three interesting questions. Is social science committed to the limiting self-doubt of cultural relativity? The answer is 'No', because 'there is a common ideal for humanity founded on laws of psychology common to all human beings'. Is illusion, contrary to Freud's assumption, a necessary safeguard for the happiness of man? The author confesses that he is uncertain how to answer this question. Finally, he argues that a liberal civilization begins when the age of ideology is over—when we have progressed beyond it.

I think it is largely because I sympathize with the author's own 'ultimate values' that I find this book intensely irritating. The main thesis of Part One is so sketchily stated and incompletely worked out that it will convince few who are not convinced already. The difficulties and limitations of applying psychoanalysis for 'the verification of ultimate values' are simply not acknowledged. I am forced to say that the author gives me the impression of not beginning to understand the point of Stevenson's *linguistic* analysis of moral discourse. His criticism of trend in Part Two is apt, but it is not new. In the sixteen pages of Part Three, the author does not succeed in doing more than tantalize us. What we require from him is a more rigorous and fuller treatment of the topics he has raised.

B. A. FARRELL

Sigmund Freud. Life and work. Vol. II. By ERNEST JONES. London: The Hogarth Press. 1955. Pp. xviii+534. 30*s*.

This volume covers the years 1901 to 1919—'Freud's years of maturity', as Jones describes them. During these years there appeared three contributions of major importance for the development of Freud's general theory, viz. *The Psychopathology of Everyday Life, Three Essays on the Theory of Sexuality* and the essay 'On Narcissism'; and it was during this period that Freud laid the foundations of the psychoanalytical theory of the psychoses in his study of the famous case of Schreber (a paranoid schizophrenic) and his essay on 'Mourning and Melancholia'. Further, the appearance of *Totem and Taboo* and the study of Leonardo da Vinci during the same period registered Freud's application of psychoanalytical theory to the wider fields of religion and art.

These years were of special importance for the psychoanalytical movement, with the fortunes of which those of Freud himself were so intimately bound; for it was during these years that the movement attained international status, and that psychoanalysis achieved, if not general acceptance, at least general recognition. Such recognition was only achieved in face of an intensity of opposition which the contemporary world has largely forgotten, but of which the reader of Jones's account will be vividly reminded. This was, however, a period of inward no less than of outward storm for the psychoanalytical movement; for it witnessed the recurrent crises which eventually led to the respective defections of Adler, Stekel and Jung. The inner history of these events, with the clash of personalities which characterized them, is described by Jones in illuminating detail; and the reader is left in no doubt as to the real issues involved. It was a critical phase in the history of the psychoanalytical movement, but one out of which eventuated the consolidation of the movement and the establishment of the characteristic psychoanalytical outlook as now understood. The dissensions which preceded the final outcome inevitably imposed a heavy strain upon Freud; and it was to relieve this strain upon him that, at the instigation of Jones, an inner Committee of six trusted analysts (including Ferenczi, Abraham and Jones himself) was established as an Old Guard round Freud to supervise the affairs of the movement and to free Freud's energies for concentration upon his researches. It may surprise even those who are familiar with the outstanding intellectual penetration, forcefulness and practical acumen of Jones to learn what a central role he played in moulding the psychoanalytical movement into the form which it assumes to-day.

As in the previous volume of the biography, the personal life of Freud receives fully as much attention as the development of his theoretical views; and, as a psychoanalyst describing the life of the founder of psychoanalysis, Jones conceives it as his duty to apply psychoanalytical insight to the interpretation of Freud's personality and the conduct of his life. The exacting nature of this self-imposed task may well be imagined, since Freud was not only an intimate friend of the author, but also, understandably, a hero in his eyes; but the task is discharged with characteristic objectivity. It would be a mistake, of course, to expect a degree of objectivity of which human nature is incapable, or even that degree of objectivity which is expected of a case-history; and there can be no doubt that, if Jones had been writing a case-history instead of a biography, the analysis of the psychoneurotic aspect of Freud's personality would have been carried considerably further. Here it may be noted that what many regard as a strain of pessimism in Freud's outlook is interpreted by Jones as an unusual degree of realism; but it is difficult

to ignore the extent of Freud's preoccupation with death as brought out in the biography—a feature which may well have some bearing on his controversial concept of 'the death instincts'.

Whilst the reader cannot help being grateful to Jones for his vivid account of the day-to-day life of a great man, it is difficult to avoid feeling that the account is sometimes burdened with excessive detail, especially where the minutiae of Freud's journeys and holidays are concerned. A case in point is the statement that on the second day of his first visit to Rome Freud 'rode in a *fiacre*, at two lire an hour, from three to seven'. The visit itself was, however, notable in that Freud described it as 'the high-point of my life'. Rome's extraordinary fascination for Freud provides an intriguing subject for speculation, especially in view of the accompanying inhibition which for many years deterred him from approaching nearer to the city, then Trasimeno, where Hannibal, with whom Freud appears to have made a powerful identification, was likewise halted. Whilst Jones regards the appeal of Rome to Freud as due at a conscious level to Freud's interest in archaeology, he interprets it at an unconscious level in terms of the symbolic significance of the Mother-City; and it is to the influence of this symbolism that he attributes the taboo implied in Freud's inhibition over entering Rome no less than the city's magical appeal. In Jones's opinion also the unconscious, no less than the conscious, appeal of Rome to Freud was strictly that of ancient Rome, as against the medieval Rome of the Holy Catholic Church. Indeed Jones finds it quite incredible that Christian Rome could have had any appeal whatever to one who spoke openly of 'the lie of salvation'. If Jones is right, it would thus appear that Freud simply rejected Christian Rome, and that his ambivalent attitude to the city was exclusively concerned with pagan Rome. This is perhaps the least convincing of all the biographer's interpretations. Thus it would appear significant in the light of psychoanalytical principles that (and here we may be grateful for some minutiae) the first place in Rome which Freud visited was, not the Coliseum, but St Peter's. Further, 'the lie of salvation' is sufficiently forceful and emotional a phrase to suggest an overcompensation—especially coming from the lips of one who devoted his life to devising something akin to a means of salvation for tortured minds. And, in addition, if Freud could be ambivalent to the pagan Rome which consciously fascinated him, it is difficult to see why he should not be equally ambivalent to the Christian Rome which consciously repelled him. His identification with Hannibal may also have a wider significance than at first appears. After all, Hannibal approached Rome as a would-be destroyer; and, if the author of *The Future of an Illusion* approached Rome as the would-be destroyer of Mother-Church, it is not difficult, in the light of psychoanalytical principles, to explain the inhibition which for so long denied to Freud entry into the Holy City.

It is interesting to note that Freud's life, as evidenced by Jones's biography, was characterized by a remarkable dualism. Not only do we find in Freud one who conceived human life as governed by the interplay of the dual forces of libido and aggression, but also one who called salvation a lie and devised a means of effecting something very like salvation, a neurologist who, whilst never abandoning the hope of ultimate physiological explanations, found it impossible to understand human behaviour in other than psychological terms, a moral puritan who described life in terms of sexuality, a philosophical stoic who was also an intellectual hedonist, a loyal Jew who hated Judaism, and a Viennese who disliked Vienna, but continued to live there until he was driven out of it. Withal, there emerges from Jones's biography the picture of one of the world's geniuses, an intellectual giant, a tireless investigator, an inveterate searcher after the truth, a man of indomitable courage and strength of purpose, a character of irreproachable integrity, a personality at once lovable and forceful, an understanding physician, a loyal friend, a faithful husband and a devoted father.

W. RONALD D. FAIRBAIRN

Mysterium Conjunctionis—Part I. By C. G. Jung. Zurich: Rascher Verlag. 1955. Pp. xv+284. 22·90 Sw. fr.

The subtitle of this latest of Jung's works, which the author actually calls his 'last', introduces it as: 'An investigation on the separation and synthesis of psychic opposites in Alchemy'. The chapters contained in this first volume are concerned with the symbolism of polarity and union.

In the last two decades Jung has been increasingly attracted by the symbolic structure of alchemical concepts and aims. While in '*Psychology and Alchemy*', which first appeared eleven years ago, he described the inner relatedness between these two fields of human endeavour, the present book is devoted to a detailed investigation of alchemical tests, which the author sets out to compare with endopsychic events as they may, for instance, appear in the analysis of dreams and active imaginations. In this task he succeeds most admirably. The reader is bound to become engrossed in the magic of the master magician, as Jung himself is so obviously fascinated by his medieval predecessors in spirit. Their aim was not, as

Jung points out, to make ordinary gold but rather to produce a lapis philosophorum, the essence of wisdom, which can only come into being through a process of separation and analysis on the one hand and of unification and growth on the other. The drama of human development is seen in the mirror image of the alchemical opus. The opposing elements in man's soul are expressed in the symbolic language of medieval mysticism. Jung foresees the possibility that later generations may feel his work to be as 'metaphoric' and symbolic as we who live to-day regard the alchemical mysticism. Characteristically he adds: 'I do not imagine that the psychological interpretation of a mysterium necessarily constitutes the *last* word.'

The book is very clear in structure. Never does the author lose sight of his theme in spite of the awe-inspiring abundance of the material presented. Ample use is made of footnotes (some of which incidentally contain pearls of great price) and the text remains lucid and simple. This is no mean achievement in view of the very involved and obscure language of a good many texts used. Yet it would not be an easy book to read for the uninitiated, that is, those who have not acquired their 'transference' to Jung before. The visionary character of Jung's work and his intellectual honesty, however, cannot fail to impress his followers and critics alike.

Dr von Franz, the co-editor, must be congratulated on a masterly use of the literature belonging to this vast subject.

W. P. KRAEMER

Psychanalyse et Conception Spiritualiste de l'Homme. By Joseph Nuttin. Publications Universitaires de Louvain. 1955. Pp. 367. Paper, 150 Belgian francs; cloth, 180 Belgian francs.

This is yet another book about Freudian psychoanalysis by a man who has not been analysed. As such books go it is quite good. Prof. Nuttin has read very carefully the works of Freud and of many of his followers, and, having high standards of intellectual honesty and accuracy, admits many of the facts they have discovered, and describes their theories clearly. In places, however, he wastes time on hair-splitting. He discusses at length two slightly different theories of sublimation, then misses the vital human problem—that many people have been taught that it is their Christian duty to love their fellow human beings, and also that sex is more or less bad and dangerous, and yet have enough introspective ability to know, at least dimly, that all love is closely related to sex. He can be very unsubtle. He writes that those who deal with psychological conflicts should have a great love of the normal, yet fails to realise that this great love is to be found only in neurotics.

The least satisfactory part of the book is Prof. Nuttin's own theory of normal psychology. This is because he tries to describe the adult without taking account of his previous development, and at the same time assumes that the psychology of children is simple and closely related to physiological mechanisms. This leaves adult psychology hanging in the air, separated from its real roots in the complex inner lives of small children, who have scarcely begun to feel the bodily and the mental as separate.

Some of his criticisms of Freud and his followers so often imply that normality is a kind of unhappy superstructure on the life of natural instinct.

Prof. Nuttin is perhaps at his best in discussing problems on the borderland of psychoanalysis and religion, such as the Christian acceptance of suffering. If he would only widen his experience by undergoing analysis himself—perhaps a Jungian one would suit his particular cast of mind better than a Freudian—he might achieve something really outstanding in this field.

J. DARROCH

Mental Health and Infant Development. Vols. I and II. By Kenneth Soddy (ed.). London: Routledge and Kegan Paul Ltd. 1955. Vol. I (Papers and Discussions), Pp. xix + 308. 25*s*. Vol. II. (Case-histories), Pp. v + 289. 25*s*.

These volumes were compiled from material presented and discussed at a Residential International Seminar held at Bishop Otter College, Chichester, in 1952 and sponsored by The World Federation of Mental Health.

International Conferences have become popular battle grounds in recent years, and of Mental Health Conferences there have been not a few: this particular gathering, however, was essentially an experiment in multi-professional education in mental health, and as such it appears to have been remarkable in a number of ways.

It was highly ambitious—as indeed are most Conferences in principle and programme—but, in reading these volumes, it becomes obvious that its ambition was in large measure achieved, viz. to provide for participants an opportunity for experiencing mutual understanding and learning, and to widen their horizons so that they might gain a greater insight into the personal and cultural problems of the people among whom they would be working.

To have, living at close quarters for three weeks, seventy-five people (of whom twenty-four were Faculty Members from Great Britain, France and America, and fifty-one participants from sixteen European countries and fourteen other parts of the world) with a variety of professional and cultural backgrounds, discussing highly controversial matters, might well have produced only a chaos of tensions and difficulties with little apparent constructive progress in any direction. So carefully, however, had the Seminar been planned, and so thoroughly had an astounding amount of preliminary work been carried out over a period of two or more years previously, that the Conference itself might be considered an outstanding example of international co-operation; while the two volumes to which the Seminar gave birth may be regarded as a monument to what may be achieved, even in the short space of three weeks, by mutual learning.

The general theme of the Seminar was that of the phenomena of family life in the first two years of a child. With such a wide range of subject-matter and such diversity of professional interests, it was inevitable that some grouping for discussion should occur. Contrary to more usual practice, the grouping arrangements were highly structured with a heterogeneity of race, language and profession. Such a grouping might well lead to relatively fruitless discussion due to an insufficient fund of common information about matters to be considered. Here again, the Chichester Seminar set an excellent example of good organization by distributing beforehand, to all participants, the individual case-histories which were to form the material for teaching and discussion.

While the centres of interest of many mental health conferences seem to lie in the abnormal rather than the normal, during the first of the three weeks at Chichester emphasis was studiedly that of normal child development, with thence a natural shift to the family as a dynamic whole vitally essential to both mental and physical well-being, though manifesting its influence somewhat differently in different cultures which, in themselves, are continuously and from generation to generation undergoing modification.

In view of the particularly rapid changes in patterns of living which have taken place within the past ten to twenty years, the Conference gave special attention to both the theoretical and practical aspects of the need for flexibility in mental health education rather than the comparative rigidity of training by didactic and traditional teaching. The false sense of security which the latter may induce can at times act as a serious hindrance to adaptability. Such a situation, incidentally, was exemplified within the Seminar itself by the cyclic pattern of some participants' own attitudes towards the discussion methods adopted.

In Vol. I are presented the lectures given to the full Conference, together with summaries of discussions arising from them and from the reports of the five special working groups; in Vol. II are presented carefully chosen representative samples of the case-histories which formed the basic material. Throughout Vol. I, frequent reference is made to material in the histories given in Vol. II; to derive the maximum value from these records, the two volumes need to be read in close conjunction. Both, incidentally, are eminently 'readable', even deceptively so in many ways for, with the commendable absence of unnecessary jargon and consequent clarity of expression, it would be easy to be misled into too rapid a reading of material so closely integrated that it presents too much substance for immediate digestion without a great deal of subsequent rumination.

The case-histories in Vol. II were intended ultimately to be made available for teaching purposes, and for this they will be invaluable, not only now, but in the future; they are, in fact, a most valuable library of documents for future research in a number of fields.

One notable point which emerged from the transactions of this Seminar was that, in a field of inquiry limited to the first two years of life, there exists a wealth of material with appeal to a variety of interests other than those ordinarily, and at first thought, associated with infant care; and that research into attendant problems becomes more fruitful and misses fewer important issues if all aspects provide contributions to our knowledge.

Whatever we do or fail to do, human society continues to exist; what lies in our power is only to help or hinder certain changes. Quotations from two papers by the anthropologist Margaret Mead seem to put the position clearly.

'However far back we go in human history we always find the family. There is no human society in the world without it and no human society has succeeded in abolishing it permanently' (Vol. I, p. 163); and,

'...we are doing the same thing to human relations as we did in the conquest of nature, plants and elements. ...One of the dangers we face...is to think of the natural scientist as someone who bends nature to his will, whereas he only proceeds by understanding the laws of nature. Once he has discovered some new law of nature, it is possible for him to work with it, to develop compensatory devices, but nevertheless always to work with it. In our own work we try to do the same thing; we try to discover something about the basic biological laws of human development, so that we can work with them.' (Vol. I, p. 184).

Despite the invaluable help given by so many groups and individuals, both in the preparation of material beforehand and in the subsequent organization and compilation, the final task of editing such a mass of data must have been heavy. Dr Kenneth Soddy has, however, made an admirable job of it, and we are indebted to him for making the work of the Seminar, which he directed, available to those who did not have the privilege of participation.

M. I. DUNSDON

A Psychological Study of a City's Cerebral Palsied Children. By ELLA B. FLOYER. London: The British Council for the Welfare of Spastics. 1955. Pp. 88. 3*s.* 6*d.*

This inexpensive little booklet is one of the best introductions to the psychological aspects of Cerebral Palsy that has appeared. The British Council for the Welfare of Spastics is to be congratulated for sponsoring its publication.

The findings have the advantage of being based on personal investigations of all the cases in the school age-range known to Liverpool Education Authority in March 1950. They therefore give a complete picture of the problem as it appears to such an authority. There are some grounds for believing that it may not give such a complete picture of the medical and social problem. An incidence of 1·3 per 1000 school-children, such as is reported, falls well short of the figure of 2 to 2·5 cases per 1000 suggested by Ingram on the basis of his Edinburgh survey. It is likely, however, that the cases omitted are, for the most part, too dull or too bright to cause educational problems.

The quantitative findings, upon which the educational recommendations for the group are based, are set out clearly. The source and numbers of the samples, from which percentages are derived, are obvious, for which a reader has reason to be grateful, in view of some deficiencies in these respects in other works in this field.

The chapter on 'Intelligence of Cerebral Palsied Children' is a very sane statement of well-established findings, and their educational implications. The vexed question of the 'unfairness' of applying tests standardized on normal children to this handicapped group is carefully and cogently considered.

The comments on 'Behavioural Deviations from the Normal' are necessarily less precisely stated, since less precisely observed, than those on intellectual assessment, but the characteristics described form such an important part of the problem that they well merit their place. They are classified as distractibility, tension, lack of drive, disinhibition, fluctuation in performance, fits, perseveration, emotional instability, emotional and social immaturity. Some tidying up of categories will no doubt result from further research, but, in the meantime, it is valuable that the possible relationship of such deviations to brain injury should be publicized. The frequent failures of such children to progress educationally at the rate their psychometric level suggests, might then be regarded as occasion for diagnosis rather than exhortation. One might, however, query the assumption that the occurrence of convulsive seizures necessarily indicates emotional instability. One might, too, have wished that space could have been found for a chapter on psychological disabilities which were neither 'general intellectual' nor those classified as 'behavioural' deviations, rather than the tantalizingly brief comments on, for example, visuo-spatial disability tucked away in the chapter on 'Some General Practical Considerations' and the Appendix.

But that is to ask for a bigger book than this modest pamphlet, which serves its purpose admirably as it is.

J. A. L. NAUGHTON

Adolescent Development. (Second edition.) By ELIZABETH B. HURLOCK. London: McGraw-Hill. 1955. Pp. xiii+590. 45*s.*

This is a new, up-to-date edition of one of the best-known text-books on adolescence. Recent research is fully reported in all the different areas, and there is special emphasis on the treatment of social class as a determinant of behaviour, juvenile delinquency, new trends in courtship behaviour, social acceptability, and the effects of physical changes on attitudes and behaviour. There is also a new chapter on family relationships.

Like all Hurlock's books, this one is characterized by its very factual tone. Quite obviously a thorough reading of the literature on adolescent development has led to a setting down of the major empirical results. There is very little theory in this book and no attempt has been made to unify in any way the data reported, or to study them from the point of view of any kind of theoretical concept. To some readers this may appear an advantage; it certainly acts as an antidote to the anecdotal theorizing so frequently indulged in by many clinical psychologists in this area.

Results are reported mainly in the form of verbal summaries; Hurlock does not reprint original tables. This, oddly enough, is justified on the grounds first that all studies cannot be reported in detail, and that it would be difficult to select some and reject others! 'Furthermore, there is a growing belief on the part of many educators that few students read the tables presented in their text-books. It would seem advisable, therefore, to eliminate them and use the space to report the important findings of the studies.' This is a debatable point. What is, perhaps, less debatable is the undesirability of the habit Hurlock shares with other American text-book writers of illustrating perfectly obvious points with photographs and diagrams which add nothing to the discussion. It is difficult to see how the discussion of family influences (page 488) is helped by a half-page photograph of a father, looking as if he had indigestion, looking quizzically at his son, who appears to be in the throes of a tooth-ache. Similarly, the contribution on page 401 of a half-page picture of a boy and a girl standing under a sign apparently advertizing a pawn-shop to a discussion of dating and courtship, in spite of its realistic reference to a money-lender, is only tangential. The whole-page cartoon on page 377 illustrates 'The seven stages in boy-girl relationships', but it is difficult to see how this will help the psychological sophistication of the reader. Devices of this kind will tend to repel rather than attract students in this country, but it would be a pity if instructors allowed these aberrations to lead them to overlook the solid contribution made by the text.

H. J. EYSENCK

Teacher, Pupil, and Task. Edited by O. A. Oeser. London: Tavistock Publications. 1955. Pp. xiii + 195. 18*s*. net.

The subtitles of this book indicate clearly enough its scope and intention. They are: 'Elements of Social Psychology applied to Education' and 'A Practical Manual for Teachers'. It is addressed to the teacher, being the contents of a course for Australian teachers, and, as such, has comparatively little interest for the psychologist. The theme is 'adjustment', and though the bulk of the book is devoted to social aspects of education, there are chapters on examinations and vocational guidance which stretch the term 'social' rather far. For the teacher, the book undoubtedly will have value, but it does not wholly escape the danger inherent in such expositions, that of implying that the findings of psychology are much simpler and more direct than they really are. A companion volume, on the theoretical foundations, is promised; this should be of more interest to the readers of this *Journal*.

JAMES MAXWELL

Psychology of Industrial Behaviour. By H. C. Smith. London: McGraw-Hill. 1955. Pp. 477. 45*s*.

This is a text-book with a message. As a *text*, it uses all the artifice and sugaring of the pill that we are accustomed to in American books nowadays. It is, in fact, very clearly set out, and refers to quite a wide range of empirical work, some of it unfamiliar. The scope is wider than that of the recent Viteles book, concentrating on the social aspects of industrial psychology, but including chapters on Tests, Human Engineering, and Training. The quality is rather uneven, the chapters on Job Satisfaction, Job Anxiety, and Union-Management relations being the most interesting. One or two sections caused the reviewer to raise his eyebrows—for example, there is a confusion of the influence of wage incentives on *production* and on *costs* (p. 33); the chapter on productivity measurement does not mention time-study; group bonus schemes are enthusiastically praised (p. 200f.), but no supporting evidence is provided; and it is maintained that the span of control should be as large as possible (p. 242f.), in the interests of decentralization. The author does dispel some of the perennial myths of popular industrial psychology—for instance, that happy workers work harder, and that supervisory training courses increase production—but he still believes that wage incentives are ineffective in comparison with 'human problems', that communications need to be improved, and that strikes are largely due to unconscious motives.

The *message* of this book is that management and unions should co-operate to reach their joint goals, and that the obstacles to reaching these are largely human problems which can be solved by applying

psychological knowledge. Insufficient attention is paid to genuine conflicts between these goals and to conflicts arising from the application of psychological knowledge: for example, evidence is cited to show that men work hardest under a medium degree of anxiety (p. 101 f.), but also that anxiety is a major source of low job satisfaction (p. 99 f.). A secondary message is that rigorous scientific methods should be used to solve the problems, though the author is not quite so aware of this when asserting his generalizations.

MICHAEL ARGYLE

Anxiety and Stress, an Interdisciplinary Study of a Life Situation. By HAROLD BASOWITZ, HAROLD PERSKY, SHELDON J. KORCHIN and ROY R. GRINKER. From the Institute for Psychosomatic and Psychiatric Research and Training of the Michael Reese Hospital, Chicago. New York: McGraw-Hill. 1955. Pp. xv + 320. Price not stated.

The difficulty about reviewing publications of this kind is that on each occasion one finds oneself saying the same thing over and over again. The opening paragraph of the authors' own preface was presumably intended to be self-explanatory: 'With the background of clinical and laboratory studies on men in war and on patients with neurotic anxiety, we undertook the present studies of healthy young American men under the stress of a real life situation—paratroop training. In the faith that somatic, psychological, affective, and behavioral processes are transactional, performances during various stressful occasions and through time are not viewed as independent entities. Rather, they are parts of the total psychosomatic-social field in constant interdependent participation and change.' Considering its brevity, it is a fair statement of the authors' psychological orientation. All too soon, however, it leads to the hope of the 'possible contribution of this conception and approach to the further development of the behavioural sciences'. What is this expectation based on? Seven groups of men, ranging from eight to thirty individuals, were repeatedly subjected during the course of military training in parachute 'jumping' to various combinations, as suggested by convention, of the following procedures: interview, questionnaire (including self-rating), projection test, Bender-Gestalt test (the cards being projected and the exposure time controlled), a tachistoscopic version of Landolt's rings, memory for digits, serial subtraction, and a number of biochemical tests (e.g. for any excess of hippuric acid in urine, for deficiency in eosinophil cells and glutathione in the blood, etc.). In spite of the labour that must have gone into the preparation of the chapter on the Intercorrelations among Experimental Procedures, no new or otherwise important psycho-physiological correlation has been discovered. Indeed, most of the biochemical indices tested had to be eventually 'interpreted to mean that these situations are not sufficiently stressful'. The main finding of psychological interest is referred to as the 'stress end phenomenon', to mean an increase in anxiety as assessed, after the completion of training. This presumably was the immediate occasion for the authors' decision to write a monograph of this length, apparently in order to 'propose an explanation that under stress individuals must exercise a maximum of psychological control in order to cope successfully with the many threats inherent in the training situation. With the training ended, such control is loosened, permitting the free expression of anxiety hitherto contained.' The authors are careful not to miss the opportunity to note that 'this is not unlike the instances reported during World War II of the development of severe anxiety among troops rotated from the front lines or combat air missions *after* they had reached rest areas removed from hazard'. So far as the reviewer's reading goes, such tendencies were not unknown to medical observers during World War I. Admittedly, in view of the fact that their study was carried out at the time of the Korean war, the authors do recognize the possibility of other interpretations. They feel, however, that the 'end phenomenon reflects a basic and theoretically important aspect of stress behaviour'—though this is as far as they are prepared to advance their argument at all. Not unexpectedly, therefore, the monograph has to end with these phrases: 'In summary, the implications of the present research have been far-reaching to those who have experienced its trials and tribulations.... The gain from the work has been less in increasing our knowledge of the details of psychosomatic problems and more in learning the complexities of psychosomatic organization and integration.... The problem of anxiety is still *non liquet*, but perhaps we know a little more why this is so.' Or, perhaps, we do not.

J. SZAFRAN

Anthropologische Psychologie. By Otto Tumlirz. Basel: Ernst Reinhardt Verlag. 1955. Pp. 469. 22 Swiss francs.

This book is the second edition, slightly changed from the original one, which appeared in 1939. It is very typical of German psychology and anthropology of the present day. There are practically no references to Anglo-Saxon work; McDougall and Watson, and the early Spearman, are almost the only non-German authors named. The discussion, altogether, is oriented very much in accordance with McDougall's instinctive psychology, and everywhere exudes the atmosphere of the *Schreibtisch*. Its only interest lies in the contrast between its completely non-experimental approach and the much more factual type of work which would be included in a book of such a title in this country.

H. J. EYSENCK

Michelangelo: a Study in the Nature of Art. By Adrian Stokes. London: Tavistock Publications Ltd. 1955. Pp. 154. 25*s*. net.

This is a short book in which the author, who is a writer on aesthetic subjects, uses Michelangelo's work as a basis for exploring the use of psychoanalytic concepts in understanding the formal qualities of art. The book consists of three short sections dealing respectively with the statement of the theory, together with a brief account of Michelangelo's life, then a discussion of the visual works, followed by comments on the poems. Each section is supplemented by notes and there are appendices on iconography and on the Medici Chapel.

This reviewer must confess to finding the book obscure and of doubtful value. Mr Stokes's thinking and his style both seem to suffer rather than profit by his somewhat uncritical excursions into Freudian literature and language. In addition, though the author writes with respect for a great artist and an interestingly complex person, Michelangelo is not well served by such poor reproductions of his works.

JOAN WYNN REEVES

OTHER PUBLICATIONS RECEIVED

Leadership and Isolation: a Study of Personality in Inter-personal Relations. By HELEN H. JENNINGS. London: Longmans, Green. 1950. Pp. xvii+349. 30*s.* (Second edition.)

Personality and Life: a Practical Guide to Personality Improvement. By LOUIS P. THORPE. London: Longmans, Green. 1941. Pp. xiii+266. 25*s.*

The Psychology of Abnormal People. By JOHN J. B. MORGAN and GEORGE D. LOVELL. London: Longmans, Green. 1948. Pp. xi+673. 35*s.*

Psychological Testing. By JAMES L. MURSELL. London: Longmans, Green. 1949. Pp. xvi+488. 32*s.* 6*d.* (Second edition.)

Expression of the Emotions in Man and Animal. By CHARLES DARWIN with an introduction by MARGARET MEAD. New York: Philosophical Library. 1955. Pp. xi+372 and added illustrations. $6.00.

Task Variability and the Level of Aspiration. By J. P. SUTCLIFFE. *Australian Journal of Psychol. Monogr. Suppl.* No. 2. Melbourne: University Press. Pp. 85. 12*s.* 6*d.*

Psychotherapy and Counseling. By L. K. FRANK, ROLLO MAY, and 46 others. *Annals of New York Academy of Sciences,* Vol. 63, Art. 3. New York: Academy of Sciences. 1955. Pp. 113. $3.50.

Pestalozzis 'Lebenskrise' und seine Auffassung vom Menschen. By JAAKKO TOIVIO. *Annals of the Finnish Academy of Sciences,* Series B, Vol. 91. Helsinki. 1955. Pp. 347. 1.200 mk.

Child Psychology. By EDNA BALINT. Cambridge University Press. 1956. Pp. 24. 2*s.* 6*d.* (A National Book League reader's guide.)

The Police and the Mental Health of Children. A study published with the assistance of UNESCO. Pp. 54. 2*s.*

Education and the Moral Basis of Citizenship: a Lecture. By ERIC JAMES. London: Heinemann. 1955. Pp. 24. 1*s.* 6*d.*

Man in his Relationships. By H. WESTMANN (ed.). London: Routledge and Kegan Paul. 1955. Pp. viii+140. 14*s.*

On the Nature of Man. By D. D. RUNES. New York: Philosophical Library. 1956. Pp. 105. $3.00.

Some Aspects of Research in Educational Psychology in the USSR. By BRIAN SIMON. London: Bulletin of Soc. for Cultural Relations with USSR, Vol. 2, No. 4. 1955. Pp. 20. 1*s.* 6*d.*

Charakter-Kunde von A bis Z. By ERICH A. OPPENHEIM. Bern: Verlag Hans Huber. 1955. Pp. 297. No price given.

Astrologie. By GUSTAV LEBZELTERN. Vienna: Franz Deuticke. 1955. Pp. 87. No price given.